PHILOSOPHICAL

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ROYAL SOCIETY

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MDCCCXXIII.

ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

CONTENTS.

APPENDIX.

Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Medal on Sir GODFREY COPLEY's Donation for the year 1822, to the Rev. WILLIAM BUCKLAND, F. R. S. Professor of Mineralogy and Geology in the University of Oxford, *&c. &c.* for his Paper printed in the Philosophical Transactions of that year.

PHILOSOPHICAL

TRANSACTIONS.

I. *The Croonian Lecture. Microscopical observations on the suspension of the muscular motions of the Vibrio Tritici.* By Francis Bauer, *Esq. F. R. S. F. L. S. and H. S.*

Read December 5, 1822.

The Croonian Lecture has usually been given by Members who had made Physiology their particular study; and I should not have ventured upon this task, had I not been encouraged by one of our Vice-Presidents, who has now, for some years, applied my microscopical observations in promoting physiological enquiries into the more minute parts of animal structure.

Without his authority, I should not have ventured to bring forward the following observations, respecting the length of time the moving powers of an animal, too small to become the object of sight without the assistance of the microscope, can have its action suspended, and again, by a change of circumstances, renewed.

This, he is induced to believe, is one of the most curious facts respecting muscular motion that has hitherto been as-

certained, and not undeserving of the notice of the Society, particularly when laid before its members as a part of this Lecture, to which it most peculiarly belongs.

This minute animal, the Vibrio Tritici, is the immediate cause of that destructive disease in wheat, known under the name of Ear Cockle, or Purples, by farmers.

On opening some of the diseased grains, I found their cavities filled with a mass of a white fibrous substance, apparently cemented together by a glutinous substance, and formed into balls, which could easily be extracted entire from the cavities of the grains, and which, when immersed in water, instantly dissolved, and displayed in the field of the microscope, hundreds of perfectly organized, extremely minute worms, all which, in less than a quarter of an hour, were in lively motion.

Having left some of these worms on a glass for five days in a perfectly dry state, they were apparently dead; but when moistened, they were again, in less than half an hour, as lively as ever.

These experiments and results were so far satisfactory, as they incontestibly established the fact, that the fibrous substance within the cavities of the diseased wheat grains, consists of real organized animals, which are endowed with the extraordinary property, of having their muscular action suspended for a considerable length of time, and of having it renewed again by the mere application of moisture: but how these animals are propagated, and how they are introduced into the cavities of the young germens, appeared to me a mystery, which I was convinced could only be unravelled by tracing them through every stage of the germination and

vegetation of the growing plant of wheat; and believing that the eggs of these worms must be conveyed into the cavities of the very young germens of the flowers of wheat by the circulating sap, in the same manner as the seed of the parasitical fungi which occasion the well-known disease in wheat, the smut-balls, and which I had, in former experiments, successfully inoculated upon sound wheat, I determined to try the same experiment with these worms. I therefore selected some sound grains of wheat, and placed some portions of the mass of worms in the grooves on the posterior sides of the grains, and planted them in the ground in the month of October, 1807. Nearly all the seeds came soon up, and I took from time to time some of the young plants for examination, but could not perceive any effect of the inoculation, till the month of March, 1808, when, in carefully slitting open the short stalk of a young plant, I found three or four worms within it; they were in every respect the same, but they were now about two-thirds larger, as well in length as in diameter.

On the 5th of June I found, for the first time, some of the worms, of different sizes, within the cavities of the young germens; and having, in the beginning of March, found some of them in an enlarged state in the stalk, I concluded that some of the original worms, with which I had inoculated the grains of seed, had got, during the germination of the grains, into the stalk, where they became mature, and laid their numerous eggs, some of which must be carried by the circulating sap into the cavities of the then forming young germens, in which the young worms extricate themselves from these eggs; and finding their proper nourishment

within the cavities of the germens, these young worms become of mature age, and lay their eggs within the cavities of these germens, which at that period nearly approach towards maturity; and these newly laid eggs, I consider to be the beginning of the third generation of the worms with which I had inoculated the grains planted in the ground in October, 1807.

Since the 5th of June, I regularly examined every second or third day an ear, to observe the progressive advancement, as well of the worms as of the germens; towards the end of June, the germens assumed various distorted forms, and began to be filled with eggs. I extracted carefully the whole contents of one of the largest grains, and putting it into water in a watch-glass, I found, on examination under the microscope, seven large worms, a great many eggs, and at least a hundred young worms, all alive, bending and twisting in the water like so many small serpents.

The natural size of the largest of these seven worms I found, by means of the micrometer, to be something more than $\frac{1}{4}$ part of an inch in length, and about $\frac{1}{80}$ part of an inch in diameter. They are more of a yellowish-white colour than the young worms, and are not so transparent; their heads are very distinct; they have a kind of proboscis, which has three or four joints, which they contract or extend like an opera glass. From the head, which is somewhat roundish, they taper gradually off towards the tail, which is scarcely half the diameter of the middle of their body, and ends in an obtuse claw-like point. At a short distance from the end of the tail is an orifice, surrounded by an elevated fleshy edge; from this orifice the worms discharge their eggs. The back

of these old worms is nearly opaque, and appears jointed, or annular; the number of joints, or rings, is from twenty-five to thirty; the belly side is more transparent; and strings of ova can be distinctly seen, through almost the whole length of the worm, to the orifice by which the eggs are discharged.

The movements of these large worms are very faint and slow; they are very seldom observed to unroll themselves entirely; they move their heads and tails faintly, but their proboscis they move constantly, extending and contracting it quickly; and when in the act of discharging their eggs, they bend the tailpiece upwards with a very quick jerk, at the passing of every egg; after having discharged all their eggs, the parent worms soon die, and in a few days they decay, and fall to pieces almost at every joint.

The eggs come out from the orifice in strings of five or six, adhering to one another at their ends, which then appear truncated; but, in water, they soon separate, and assume an oval form, which, in its middle, is slightly contracted. These eggs consist of an extremely thin and transparent membrane, through which the young worm can be distinctly seen; and, if attentively observed, it may be seen moving within this envelope. The egg is about $\frac{1}{300}$ part of an inch in length, and $\frac{1}{800}$ or $\frac{1}{900}$ part of an inch in diameter.

In about an hour and a half after the egg is laid in water, the young worm begins to extricate itself from the egg. One extremity of the worm (which I consider to be the head) comes out at one end of the egg; and by continual twisting and active exertion, the young worm comes gradually entirely out. I watched one individual from the first appearance of its head till it was entirely extricated, the operation was

effected in one hour and twelve minutes. The eggs, after the worms have quitted them, soon shrivel and decay, and it seems they ultimately dissolve, as in a very few days they entirely disappear, as well those in the water, as those that have been hatched within the germens.

The young worms are somewhat smaller and more transparent than those which are found in the more mature grains, but in a very short time after they have mixed with the others, they cannot be distinguished from them. Those which are found in the cavities of the mature grains, are nearly all of the same size; they are from $\frac{1}{33}$ to $\frac{1}{36}$ part of an inch in length, and $\frac{1}{1200}$ part of an inch in diameter. They are milk-white, semi-transparent; and if viewed with the strongest magnifying power, appear annular, like the large worms, though no external indentations are observable; they appear like fine glass tubes filled with water, and containing many air bubbles in close succession, and of the same number as the rings or joints in the old worms. At both extremities (one of which is more sharply pointed than the other), there are no such divisions or joints perceptible. These extremities are each about $\frac{1}{8}$ of the whole length of the worm; they are perfectly transparent, and appear like solid glass.

Respecting the sex of these minute animals, I could never discover any external distinction. The old worms in the same germen are almost every one of a different size; they have all the same proboscis and the same orifices. Three of the seven worms from the same grain which I first examined, were laying their eggs at the same time, though they were not of precisely the same size; but the other four did not; they were considerably smaller, and evidently much

younger; but I have not the least doubt, had they been left undisturbed in the grain, they would, at the proper period, have attained the same size as the others, and would have produced eggs.

This opinion I consider confirmed by my subsequent investigations of grains approaching nearer to maturity; in them there was no such striking difference in size; at that period, the old worms in the same grains, which probably laid their eggs first, were now in a decaying state; some parent worms were found dead, and those still alive were laying eggs, and of the same size as those which I had before observed in the act of discharging their eggs. I also found that the infected germens in the upper part of the ears very frequently contained only one single large worm; notwithstanding which, these germens were gradually filling with eggs, in the same manner as those in which originally there was more than one worm; and among the diseased germens of plants which I had inoculated with the worms and the fungi of the smut-balls, both diseases having taken effect, I found several germens containing only two or three large worms, which formed as many distinct nests within the same germen, having each of them a large distinct cluster of eggs, kept separate by the fungi of the smut-balls that occupied the cavities within those germens.

From all the observations I had an opportunity of making, it appears, that there is no distinction of sex, and that they are true hermaphrodites.

The latter end of July, the diseased grains had almost all attained their full size, and assumed a brownish tint; and about the fifth of August they were all of a dark brown co-

lour, variously distorted, and as hard as wood. The cavities of these grains were now completely filled with young worms, and these worms were, in every respect, the same as those with which I had inoculated my first seed grains; and those specimens were now more than twelve months old, and, consequently, the grains and the worms within them were completely dry; but after soaking them in water about an hour, the worms recovered their powers of moving, and were again as lively as those which were taken from the living plants.

These experiments I repeated with grains from the same specimens, for five years and eight months, always with the same success; but I observed that the longer the grains were kept dry, the longer they required to remain in water before the worms recovered their motion; but after the expiration of five years and eight months the worms were really dead.

The worms of the specimens which were the produce of my inoculated plants, retained their reviviscent quality for six years and one month; and this is the longest time of suspension I have hitherto ascertained; after that time the power of resuscitation ceased.

The large worms, after they become dry, die, and never revive; neither can the young worms within the eggs be revived, if the eggs have been but for a moment dry before the worms have extricated themselves.

Experiments with such worms as had been revived in water before, and had been dried again, I repeated many times; I soon found that those which had been kept the shortest time in water, recovered their motions soonest; so that those which had been examined on the plain object-glass where

only a very small quantity of water can be applied, which very soon evaporates, almost every individual worm recovered in less than a quarter of an hour; and if the water is a second time suffered soon to evaporate, the experiment may be repeated many times successfully with the same worms; but after the second or third repetition, if there is a suspension of a week or ten days at each interval, several worms do not revive, and the number of these increases at every succeeding repetition. If this experiment be not repeated too soon or too frequently, the worms retain their reviviscent quality much longer; the longest period of recovery, after a second suspension, I have hitherto ascertained, was eight months.

If the worms are kept alive in water for a week or ten days, the experiment cannot be repeated so often, but the intervals of suspension may be prolonged considerably. I made the experiment very recently with grains which were three years and ten days old, and dry. After extracting the worms from the grains, I kept them in water thirty-five days, and after they had again been fifteen days perfectly dry, I supplied them with water, and in less than twelve hours soaking they were again, almost every individual, in as lively motion as if they had just been taken from fresh grains of the growing plant. I had the pleasure of showing these worms, in that state, to several Members of the Society, on the 29th of September last; after that day, I preserved the same specimens eighteen days, perfectly dry; when, supplying them with water, I found, in less than three hours, at least one-third of them in lively motion; but the next morning,

after they had just been sixteen hours in water, they were all dead.

If these worms are kept in a large glass, where the water cannot evaporate, they remain alive more than three months, but then they gradually die, and become as straight as needles; in that state they remain unaltered in size and shape, for more than fourteen months; and even after that time I found only a few floating on the surface of the water, in a state of decay; they were then much thinner than they had originally been, and were shrivelled at all their joints, the number of which could now be distinctly ascertained; the worms then assume a brownish colour, and at the least touch, or the slightest agitation of the water in which they are kept, they fall to pieces almost at every joint.

If the worms of one grain are put into water in a watch glass, they generally separate, and spread over a surface of about an inch in diameter, but during night, or if kept some hours in a dark place, they all assemble again, and entwist themselves together in a round cluster, the same as they originally formed within the cavity of the grain; the same glutinous substance by which they were cemented together whilst within the grain, surrounds and envelopes them again; and if they are suffered to get dry in that state, they retain their reviviscent property for as long a time as if they had been preserved within the grain.

The above mentioned glutinous substance appears to be of an oily nature, for if a cluster of the worms be extracted from the grains, and be slightly rubbed on the object-glass, it leaves a stain on the glass, which, if viewed through the

microscope, appears to consist entirely of a clear and colourless fluid, which neither evaporates nor dries on the glass after several months; but if the cemented mass of worms is immersed in water, the clear fluid almost instantaneously dissolves, and the worms separate.

If the worms are kept in a considerable quantity of water, and the water is frequently changed, the worms very soon die in the water, or if taken out whilst yet alive, and suffered to dry on the glass, they remain dead; but if the young worms are kept only in a moderate quantity of water in a watch-glass, the mucus, or glutinous substance, rises, and, in about twelve hours, forms a film on the surface of the water, and soon becomes nearly opaque, and sinks again upon the worms at the bottom of the glass, and in that state the worms continue alive more than two months; but if that film be carefully scummed off, the worms in the water die in less than twelve hours.

This glutinous substance must be secreted by the worms; since in grains in which the worms and the fungi or smut-balls exist, that portion of the cellular tissue of the young germens, where a worm has formed its nest and laid its eggs, is entirely preserved; whilst in those portions of the grains which are immediately in contact with the fungi, the cellular tissue entirely disappears, and the fungi are only enveloped by the external tunic of the young germen.

From these facts, we are to consider this glutinous substance as the probable cause of preserving these minute animals for such a length of time. What is recorded of the shell-snail, which can, by its own mucus, hermetically seal itself for thirty years in its shell against a wall, is similar to this: when

the mucus is dissolved, the air in the lungs is rarified, and forces its way out, so that fresh air rushes into the lungs, and it recovers.

I must however state, that this mucus continues to exist in grains of wheat now more than twenty years old, though the worms within them have, more than twelve years ago, lost their reviviscent quality; but whether length of time can, or cannot, effect such chemical change in the nature of that mucus, and by what means it has lost this preserving power, I must leave for others to decide.

Since writing these observations, I find that a great deal has been written on the subject by authors of eminence, and I have had an opportunity of reading the works of the undermentioned writers.* After an attentive perusal of their

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* TURBERVIL NEEDHAM appears to have been the first discoverer of this extraordinary animal. In his "Microscopical Observations on the Worms discovered in "Smutty Corn," published in 1744, in the XLII. volume of the Philosophical Transactions; and in a small volume, under the title of "An Account of some New Microscopical Discoveries," &c. &c. published in London, 1745, he most correctly describes these worms, and their œconomy, illustrating his description by very correct figures; yet, in a subsequent publication, he most unaccountably retracts every thing he had before stated respecting them, and declares the white fibrous substance to be true zoophytes.

MAURICE ROFFREDI, in his "Memoir sur l'Origine des petits Vers ou Anguilles "du Bled Rachitique" published in the 5th volume of the Journal de Physique, 1775, has also written fully on this subject. He seems to have attentively observed the whole œconomy and peculiarities of these minute animals, in all the stages of their existence, but fell into a very great error, mistaking worms found in the stalk of a sickly young plant of wheat, for the same as those with which he had inoculated his seed grains. The worms he found, constitute a distinct disease in corn, a detailed account of which, with illustrations, is among my original drawings of all the diseases in corn, now deposited in the Banksian Library. In the same 5th volume of the Journal de Physique, page 197, ROFFREDI published a second memoir on

observations, I feel great satisfaction in finding that I have nothing to add, nor any alterations to make, in my own investigations and illustrations.

EXPLANATION OF PLATES.

PLATE I.

Fig. 1. A full grown diseased ear of white wheat, natural size.

Fig. 2. A single spiket of an unripe diseased ear in a green state; magnified 5 diameters.

Fig. 3. An infected young germen from the upper part of the green spiket; magnified 10 diameters.

Fig. 4. Transverse section of the same, with one single large worm in its cavity, but no eggs, magnified 10 diameters.

Fig. 5. An infected young germen from the lower part of the same green spiket; magnified 10 diameters.

the same subject, in which he does not offer any new facts respecting these worms, except that he successfully inoculated them upon grains of rye and barley. In the 7th volume of the Journal de Physique, published in 1776, page 369, ROFFREDI gives a third memoir on the subject, chiefly intended to clear up the confusion occasioned by many authors giving a different name to the same disease.

In the 7th volume of the Journal de Physique, published in 1776, page 43, FELIX FONTANA gives a long letter on the subject of these worms; but his chief object is, to establish two most erroneous ideas; first, he maintains that the infected grains in which the worms are found, are extraneous tumors, or gall-nuts, the mere produce of the worms; this, however, to every one who has seen one of the infected grains, must appear totally at variance with the fact. Secondly, that the suspension of the muscular motions of these worms, which is extended to such extraordinary length of time, is not a state of torpor, but real death, and extinction of life; that the worms really die as often as they get dry, and are again brought to real life, as often as they are moistened with water.

Fig. 6. Transverse section of the same, having one worm and many eggs in its cavity; magnified 10 diameters.

Fig. 7. An infected young germen in a more advanced state; magnified 10 diameters.

Fig. 8. Transverse section of the same, having several large worms, a great many eggs, and some young worms in its cavity; magnified 10 diameters.

Fig. 9. An infected and considerably distorted germen at its full size; magnified 10 diameters.

Fig. 10. Transverse section of the same, its cavity entirely filled with young worms; magnified 10 diameters.

Fig. 11. A single spiket of the ripe ear, containing four infected grains; magnified 10 diameters.

Fig. 12. One of the infected grains of the upper valves of the same spiket; magnified 10 diameters.

Fig. 13. Transverse section of the same, containing in its cavity young worms only; magnified 10 diameters.

Fig. 14. The uppermost, and smallest infected grain of the same spiket; magnified 10 diameters.

Fig. 15. Transverse section of the same; magnified 10 diameters.

Fig. 16. The lowermost and largest infected grain from the same spiket; it is nearly divided into two lobes and two cavities; magnified 10 diameters.

Fig. 17. Transverse section of the same; magnified 10 diameters.

Fig. 18. The infected grain next in order to the preceding one, on the same spiket; magnified 10 diameters.

Fig. 19. Transverse section of the same; magnified 10 diameters.

Fig. 20. An infected grain, the plant of which had been inoculated with the worms, and the uredo fœtida, or smut-balls, and both diseases had taken effect; magnified 10 diameters.

Fig. 21. Transverse section of the same; containing in its cavity some large worms, many eggs in distinct clusters, and the rest filled with uredo fœtida, or smut-balls; magnified 10 diameters.

Fig. 22. Two infected grains found in one and the same valve of an ear, the seed of which plant had likewise been inoculated with both diseases; one grain, A, is infected with the worms and the uredo, and the other with the uredo fœtida only, see B, therefore a true smut-ball; each of these grains has the two rudiments of the pistils at their summits; magnified 10 diameters.

Fig. 23. Transverse section of the same. In the cavity of the grain A, are two large worms, and many eggs in two distinct clusters, and the rest of the cavity is filled with the uredo, and the cavity of the other grain, B, is filled with the uredo fœtida only; magnified 10 diameters.

PLATE II.

Fig. 1. A group of worms in water, as seen in the field of the microscope. At A, is one of the largest worms in its most usual attitude, and in the act of laying its eggs; at B, is one of the smallest old worms; at C, D, E, F, and G, is represented the manner of a young worm extricating itself from the egg; at H, is the empty egg-shell; at I, is a dead worm; the rest are worms, bending and twisting themselves

in their most usual attitudes; besides some eggs full and some empty; magnified 100 diameters.

Fig. 2. At A, is an egg containing a living young worm, twisted and rolled up in its natural manner; at B, is an egg from which the worm has recently issued; at C, is a decaying and shriveling egg; at D, D, D, are young worms; at E, is a dead worm, stretched out as is always the case; at F, is a dead worm, which had been kept 14 months in water, and only then just began to decay. All the objects of this figure are magnified 200 diameters.

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Fig. 1.

Fig. 2.

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II. *On Metallic Titanium.* By W. H. Wollaston, *M. D. V. P. R. S.*

Read December 12, 1822.

The evidence that we yet possess of the reduction of titanium to its metallic state, is not altogether satisfactory; for even Laugier, (who has described a valuable series of experiments made upon it in 1814, and who had the advantage of all the previous knowledge acquired by the labours of Vauquelin and Hecht, in 1796, of Lowitz, in 1798, and of Lampadius, in 1803), could only say that he thought himself justified in considering certain parts of his product which were of a golden colour as really reduced; adding in confirmation, that Messrs. Vauquelin and Haüy, to whom he had shown them, "appeared disposed to adopt his opinion."*

As M. Laugier had not the means of confirming his opinion by analysis, I may presume that an account of some experiments which I have recently made upon this substance, will be acceptable to chemists in general; and that in proportion to the degree of doubt they may entertain, they will feel interested to examine scrupulously the evidence I shall adduce as to the metallic state of the subject of my experiments.

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* Je me crois fondé à regarder cette couche mammelonnée comme la portion réellement réduite - - - - - - - - - - - - - - - -
M. M. Vauquelin et Haüy m'ont paru disposées à adopter cette opinion.
Ann. de Chimie, V. 89, p. 317.

My attention has been directed, by various friends, especially by Professor BUCKLAND, who gave me the subject of my experiments, to certain very small cubes, having the lustre of burnished copper, that occasionally are found in the slag of the great iron-works at Merthyr Tydvil, in Wales, which, from their hue, have, by some persons, been imagined to be pyritical. Their colour, however, is not truly that of any sulphuret of iron that I have seen; and though the form be cubic, it is not the striated cube of common iron pyrites, which so often passes into the pentagonal dodecahedron, but similar to that of common salt; for any marks, that are to be discerned on their surfaces, appear as indented squares instead of striæ.

Their hardness also is totally different from that of pyrites, and is such as, when combined with the preceding characters, marks a substance wholly unknown to mineralogists. By selecting a sharp angle of one of these cubes, I found that I could not only write upon the hardest steel, or upon crown glass, but could even visibly scratch a polished surface of agate or rock crystal.

Having broken out some of these crystals for experiment, I found them all apparently attracted by a magnet; but observing that they had still small portions of slag adherent to them, they were next digested in muriatic acid, which, by dissolving the iron from their surfaces, soon freed them from their deceptive appearance of magnetism.

The cubes thus purified are not acted upon by muriatic acid.

Nitric acid has no action upon them.

Nitro-muriatic acid does not dissolve them.

Boiling sulphuric acid does not affect them.

Before the blow-pipe they are utterly infusible. A con-

tinued heat oxidates them, and they become purple or red at the surface, according to the degree of oxidation, or depth to which it penetrates.

Borax has no action upon them, but only cleans the surface from any oxide that may be formed. Neither does the addition of subcarbonate of soda produce more effect than borax alone.

Nitre, aided by a strong heat, oxidates them rapidly ; but unless the heat be long continued, the effect is only superficial.

The combined action of nitre and borax together, soon effects their solution, as the latter dissolves the oxide as fast as it is formed, and presents a clean surface for fresh oxidation. But as these salts do not unite by fusion, the addition of soda, as a medium of union, considerably shortens the process. The fused mass becomes opake in cooling, by the deposit of a white oxide, which may either be previously freed of the salts by boiling water and then dissolved in muriatic acid, or the whole mass may be at once dissolved together.

In either case alkalies precipitate from the solution a white oxide, which is not soluble by excess of alkali, either pure, or in the state of carbonate. By evaporating the muriatic solution of the oxide to dryness, at the heat of boiling water, it is freed of any redundant acid, and the muriate which remains is perfectly soluble in water, and in a state most favourable for exhibiting the characteristic properties of the metal.

Infusion of galls gives the well known colour of gallate of titanium. The colour occasioned by adding triple prussiate of potash is red, as observed by LAUGIER, and so nearly re-

sembling that of the gallate, that I do not think any difference that I can discern is to be depended upon as constant. It differs from prussiate of copper by inclining to orange instead of purple, while the colour of prussiate of uranium is rather brown than red.

Since the oxide thus examined agrees in its characteristic properties with that of titanium procured from Anatase, I cannot entertain a doubt as to the general nature of the substance under consideration. I believe it to be pure, for I find no trace of any other substance combined with it, not even of iron, although the crystals are found imbedded in an iron slag, in the presence of metallic iron; nor yet of silica, for which the oxide has a strong affinity. Neither is there any sulphur present, as the salt which remains after oxidation of it by nitre, contains no trace of sulphuric acid.

That the cubes are in the metallic state, is nearly proved by their lustre, by the effect of nitre upon them, and by the failure of borax to act upon them, till they have been subjected to the action of nitre. It may be farther observed, that, when the action of nitre is rapid, heat is evidently generated, as by the combustion of other metals; but as I acted upon them in their solid state, and did not pulverise them, I did not witness what could properly be called detonation, as described by Lampadius.

The property which may be regarded as most decisive of the metallic state of these cubes, is the power which I find them to possess of perfectly conducting the most feeble electricity.

If a slip of zinc and another of copper be placed in contact, and immersed together in dilute sulphuric acid, bubbles of

gas are seen to rise from the surfaces of both the metals; but, if a piece of paper be interposed between them, then no gas is given off by the copper. In a piece of paper, so placed between zinc and copper, I made a small hole, and after inserting in it one of the cubes so as to be in contact with both the metals, I had the satisfaction to find an electric communication completely established by this interposition, for gas was now given off from the surface of the copper.

From the situation in which this metal is found, it evidently has no affinity for iron in the metallic state, and it seems equally indisposed to unite with every other metal that I have tried. Though it is evidently impossible to measure with precision the specific gravity of such specimens as I first received for analysis, I was in hopes of trying whether one of the largest of the cubes would sink or swim in melted tin, and for that purpose endeavoured to tin its surface, but I could not succeed in uniting it with either tin or lead, with silver or copper, and had no encouragement to prosecute farther a series of negative results, in search of metals for which it may have an affinity.

From the extreme infusibility of these cubes, it seems probable that they have not been formed by crystallization in cooling from a state of fusion, but have received their successive increments by reduction of the oxide dissolved in the slag around them: a mode of formation to which we must have recourse for conceiving rightly the formation in nature of many other metallic crystals.

Since the date of this communication, the liberality of Mr. ANTHONY HILL, of Merthyr Tydvil, has supplied me with a

larger quantity of the slag which formed the subject of my first experiments, and has enabled me to determine the specific gravity of metallic titanium to be 5.3. For this purpose, the vitreous part was fused with a mixture of borax and subcarbonate of soda in about equal quantities, and was then dissolved in muriatic acid, which also removed a quantity of metallic iron, and left the titanium freed from extraneous matter. Though great part of what was thus obtained from the interior of the slag was in a pulverulent state, the quantity, which amounted to 32 grains, and displaced 6,04 of water, was sufficient to preclude any considerable error.

I have moreover learned that metallic cubes, similar to those which I have above described and examined, were, more than 20 years since, observed in a slag at the Clyde iron-works in Scotland; that a small quantity has also been met with at the Low Moor iron-works, near Bradford, in Yorkshire; and at the Pidding iron-works, near Alfreton, in Derbyshire; and that some good specimens have been obtained from Ponty-pool, in Monmouthshire; but it does not appear that any one has ascertained, or even suspected, the real nature of this singular product.

III. *On the difference of structure between the human Membrana Tympani and that of the Elephant. By Sir* Everard Home, *Bart. V. P. R. S.*

Read December 12, 1822.

In the year 1799, I brought before the Society proofs of muscularity in the membrana tympani of the elephant, and led on by that discovery, I was enabled to show that this membrane in the human ear is also muscular.

As the organ I examined in the elephant was in a dried state, and the parts somewhat distorted, I went no farther than to state, that the form of the membrane was oval; the fibres, as in the human ear, radiated from the circumference to the centre, and instead of being attached to the point of the handle of the malleus, were connected to it through its whole length: that this oval form, common to many quadrupeds, as the horse, deer, and cat, was the probable cause of these animals not having their ears adapted to musical sounds, in the same degree with man, whose membrane is circular, all the muscular fibres forming radii of equal lengths from the centre to the circumference. Ever since that time, I have put my friends, in the different parts of India, under contribution to supply me with the head of a young elephant preserved in spirit, to enable me, with more accuracy, to examine the fibres of the membrana tympani.

In this object I have at last succeeded, through the kindness

of Sir STAMFORD RAFFLES, whose exertions in collecting specimens of natural history, as they are beyond all example, so they are above my praise; they are known to this Society, and there is no doubt, that many new opportunities will occur of their being recorded in the Philosophical Transactions.

The elephant was only three weeks old, and one ear has the membrana tympani in a perfect state, and in its natural situation. It is of an oval form, one inch and half long, one inch $\frac{1}{8}$ broad; the muscular fibres lie upon the inner surface of the membrane; the handle of the malleus lies transversely, and passes in the direction of one of the foci of the oval; it terminates in the centre of that focus; the muscular fibres not only terminate by an attachment to the point, but are connected to the two sides. From this arrangement, one portion of the fibres is short, the other more than double their length; this will be better understood by the annexed drawing.

So great a difference in the form and structure of this membrane in the elephant, from that of the human ear, makes it obvious that this animal cannot adapt its ear to musical sounds in the same manner the human ear does, the fibres being of such different lengths; it became therefore a consideration, what purpose was answered by such a disproportion in the length of the fibres.

Having heard from my friend Mr. CORSE, who had attended to the habits of the elephant, that they heard sounds at a great distance, an instance of which, respecting the call of the young, is inserted in my former paper, I was induced to believe that the long fibres answered that purpose.

To see the effect of high and low notes upon the elephant

in Exeter 'Change, Mr. Broadwood kindly sent one of his tuners with a piano-forte to make the experiment: the higher notes hardly attracted notice, but the low ones called up the elephant's attention. He brought his broad ears forward, remained evidently listening, and he made use of sounds rather expressive of satisfaction than otherwise.

The full sound of the French horn produced the same effect.

The nearest approach I have met with among quadrupeds to this peculiarity in the elephant, is in neat cattle: in them the membrane is more oval proportionably than in the elephant; it is $\frac{10}{20}$ of an inch long, $\frac{8}{20}$ broad. The handle of the malleus lies in the direction of the transverse diameter of the oval, and extends $\frac{2}{3}$ of its length: it is not, however, situated in the middle line of the oval, but so much nearer to the anterior side, that the fibres on that side are $\frac{2}{3}$ shorter than those on the opposite.

In the deer, the membrane is of an oval form, whose transverse diameter is $\frac{7}{20}$ of an inch, the conjugate $\frac{5}{20}$: the malleus has its handle nearer the middle line than in neat cattle, the anterior fibres are $\frac{2}{20}$ of an inch, the posterior $\frac{3}{20}$ of an inch long. This is seen in the drawings. [Plate V.]

In the horse, and hare, the handle of the malleus lies in the middle line, so that the fibres on the two sides are equal. In the hare, the handle is more curved. See the drawing. [Plate V.]

In the cat, the fibres are nearly the same as in the horse. I mention this circumstance, since it leads to the conclusion, that the whole of the feline kind have a similarly constructed organ.

The effect of the high notes of the piano-forte upon the great lion in Exeter 'Change, only called his attention, which was very great. He remained silent and motionless; but no sooner were the flat notes sounded, than he sprung up, endeavoured to break loose, lashed his tail, and appeared to be enraged and furious, so much so as to alarm the female spectators. This was accompanied with the deepest yells, which ceased with the music.

EXPLANATION OF THE PLATES.

Plate III. The membrana tympani.

- Fig. 1. Human Membrana tympani; natural size.
- Fig. 2. Ditto magnified.
- Fig. 3. Ditto in the elephant; natural size.

Plate IV. Membrana tympani in situ; and mastoid cells, of the elephant.

Plate V. The membrana tympani.

- Fig. 1. In neat cattle; natural size.
- Fig. 2. Ditto magnified.
- Fig. 3. In the deer; natural size.
- Fig. 4. Ditto magnified.
- Fig. 5. In the hare; natural size.
- Fig. 6. Ditto magnified.

Fig. 2.

Fig. 1.

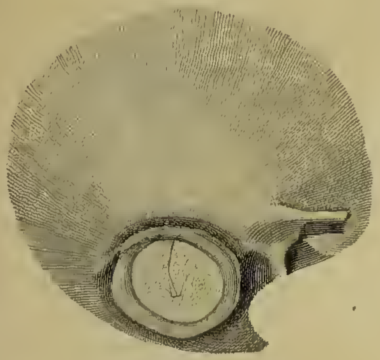


Fig. 3

J^s Basire sc.

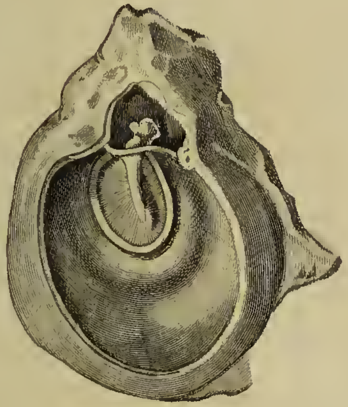
Js Basire sc.

Fig. 1.

COW.

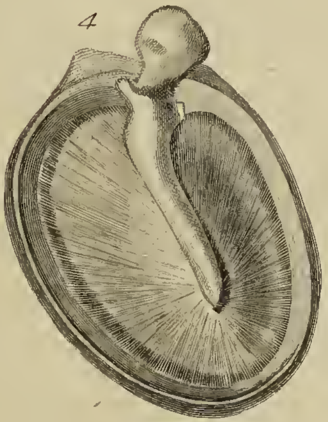
2

3



DEER.

4



5

HARE.

6

Js Basire sc.

IV. *Corrections applied to the Great Meridional Arc, extending from latitude* 8° 9′ 38″,39, *to latitude* 18° 3′ 23″,64, *to reduce it to the Parliamentary Standard. By Lieutenant Colonel* W. Lambton, *F. R. S. and Corresponding Member of the Royal Academy of Sciences at Paris.*

Read January 9, 1823.

I have recently received from Captain Kater a printed paper from the Philosophical Transactions for 1821, giving an account of his experiments in examining and comparing the different standard scales. I have read with great attention and satisfaction the whole of his results, and am glad to find that the Commissioners for considering the subject of weights and measures, have adopted Mr. Bird's scale of 1760, as by that means there is now a universal standard of comparison, which applies to the French metre, and to all the measures used on the Continent.

From Captain Kater's results it appears, that my standard scale requires a multiplier of —,000018 to make it agree with the above scale of Mr. Bird; and that Ramsden's bar, used in the Trigonometrical Survey of Great Britain, requires the multiplier +,00007. That is to say, with respect to a measurement on the meridian, the degree depending on my brass scale must be multiplied by, 000018 and the product subtracted from the measure given by the scale, to reduce it to what it would have been, had it been measured by

what is now the parliamentary standard ; and the degree depending on Ramsden's bar, must be multiplied by ,00007, and the product added to the measure given by the bar, to reduce it to the standard measure.

Now the arc which I have measured depends on both these standards, as I had a new chain sent out by Mr. Berge in 1802, which was laid off from Ramsden's bar at the temperature of 52° ; and this chain has never been used as a measuring chain, but as a standard chain with which to compare the other, till after the base near Gooty was measured, when the irregularity in the wear of the measuring chain was first discovered; for then the brass standard *scale* was had recourse to, and a correction applied to the base ; and the triangles computed back to Yerracondah, one of the meridional stations about half way between the base near Bangalore and that near Gooty : and this is the correction alluded to in page 487 in the second part of the Philosophical Transactions for 1818. It thence follows, that the meridional distance from Yerracondah to Daumergidda depends on the brass standard *scale;* and that the meridional distance from Yerracondah to Punnae, near Cape Comorin, depends on the standard *chain*.

I shall now proceed to give the different sections of the arc, correcting them by the above factors as I go along.

	Feet.
From Namthabad back to Yerracondah, by the base, as measured in 1811. - - -	429120,6 *s*.
The same distance by the corrected base, is -	429134,3 *s*.
Hence, from Namthabad to Yerracondah, depending on the standard *scale*, is - -	429134,3 *s*.
From Yerracondah to Dodagoontah, depending on the standard *chain*, is - - -	332662,3 *s*.

Feet.

From Dodagoontah to Putchapolliam, depending on the standard *chain*, is - - - 727334, s.

These three being corrected by their respective factors, we shall have the meridional distance from Namthabad to Yerracondah - 429126,6 s.

From Yerracondah to Dodagoontah - - 332685,6 s.

From Dodagoontah to Putchapolliam - - 727385,5 s.

From Namthabad to Putchapolliam - - 1489197,7 s.

or Fathoms 248199,6 s.

Now the celestial arc between Namthabad and Putchapolliam is $4^\circ\, 6'\, 11'',28 = 4^\circ,10313$.

Hence $\frac{248199.6 \text{ Fathoms.}}{4^\circ.10313} = 60490,31$ the degree due to $13^\circ\, 2'\, 55''$, the middle point.

Fathoms.

Then from Putchapolliam to Punnae, depending on the standard *chain*, is - - - 171516,75 s.

Which, corrected by its multiplier, is - - 171528,76 s.

And the celestial arc, is $2^\circ 50' 10'',54 = 2^\circ,83626$

Hence $\frac{171528,76}{2^\circ,83626} = 60477,09$ fathoms, the degree due to $9^\circ\, 34'\, 44''$ the middle point.

Then from Namthabad to Daumergidda, depending on the standard *scale*, is - - 178904,7

Which, corrected by its multiplier, is - - 178901,48

The celestial arc is $2^\circ\, 57'\, 23'' = 2^\circ,95648$.

Hence $\frac{178901.48}{2^\circ.95648} = 60511,65$ fathoms, the degree due to $16^\circ\, 34'\, 42''$ the middle point. From which we have the degrees as follows :

	° ′ ″	Fathoms.	
The degree for latitude	9 34 44	= 60477,09	Indian.
for latitude	13 2 55	= 60490,31	
for latitude	16 34 42	= 60511,65	

	° ′ ″		Fathoms.	
The degree for latitude	47 30 46	=	60779,00	French.
for latitude	52 2 20	=	60824,26	English.
for latitude	66 20 12	=	60955,00	Swedish.

Then computing from Eq. 3, page 498, in the Philosophical Transactions for 1818, 2d. Part, we shall have the ellipticity of the earth as follows:

By the Indian and French $\frac{1}{310{,}07}$; $\frac{1}{309{,}64}$; $\frac{1}{313{,}73}$; Mean $\frac{1}{311{,}15}$

By the Indian and English $\frac{1}{310{,}3}$; $\frac{1}{309{,}94}$; $\frac{1}{313{,}72}$; . . . $\frac{1}{311{,}32}$

By the Indian and Swedish $\frac{1}{307{,}88}$; $\frac{1}{307{,}55}$; $\frac{1}{309{,}92}$; . . . $\frac{1}{308{,}45}$

General Mean $\frac{1}{310{,}31}$

	Fathoms.
Now half the terrestrial arc between Putchapolliam and Punnae, is - - - -	85764,38
To which add half a degree south, or - -	30238,54
Their sum is the terrestrial arc between Putchapolliam and half a degree south of the middle point - - - - - - -	116002,92
The latitude of half a degree south of the middle point, is 9°4′44″, or more correctly 9°4′43″,66, which is the latitude of the south extremity of an arc of complete degrees.	
Now the terrestrial arc between Putchapolliam and Namthabad, is - - - - -	248199,62
And between Namthabad and Daumergidda -	178901,48
The sum of these *three* arcs is the terrestrial arc between latitude 9°4′44″ and Daumergidda	543104,02

The latitude of Daumergidda is 18° 3′ 23″,58

From which subtract - - - 9 4 43 ,66

Their difference or arc = - - - - 8° 58′ 39″,92 whose measure is - 543104,02
To which add - - - - - - 0 1 20 ,08 whose measure is - 1346,08

Gives the No. (*n*) of complete degrees = 9 0 0 ,00 whose measure (A) = 544450,10

Then, referring to page 509 Philosophical Transactions for 1818, Part II., we have $n = 9$; $A = 544450,1$; $a = (\sin^2.\overset{(2)}{l} - \sin^2.\overset{(1)}{l}) = ,006042 \cdot b = (\sin^2.\overset{(2)}{l} - \sin^2.\overset{(1)}{l}) + (\sin^2.\overset{(3)}{l} - \sin^2.\overset{(1)}{l}) + \&c. = ,263137$; $m' = \frac{310,31 \cdot A}{36 + 310,31 \cdot n} = 60477,76$; $A - n m' = 150,26$; $d = (A - m'n) \cdot \frac{a}{b} = 3,45$ and $Q = 571$ nearly.

From which the following Table has been computed; and it appears from *this table*, that the first degree in latitude 9° 34′ 44″ by the measurement is 0,67 fathoms in defect; and that the degree in latitude 16° 34′ 42″ (which may be taken for 16°34′44″) by the measurement is 3,21 fathoms in excess.

TABLE.

	Degrees in Fathoms.	Latitude.
		° ′ ″
$\overset{(1)}{m} = \overset{(1)}{m} + o$ - - - - - - - - -	60477,76	9 34 44
$\overset{(2)}{m} = \overset{(1)}{m} + d$ - - - - - - - - -	60481,21	10 34 44
$\overset{(3)}{m} = \overset{(1)}{m} + Q(\sin^2.\overset{(3)}{l} - \sin.^2\overset{(1)}{l})$ - - -	60484,95	11 34 44
$\overset{(4)}{m} = \overset{(1)}{m} + Q(\sin^2.\overset{(4)}{l} - \sin^2.\overset{(1)}{l})$ - - -	60489,03	12 34 44
$\overset{(5)}{m} = \overset{(1)}{m} + Q(\sin^2.\overset{(5)}{l} - \sin^2.\overset{(1)}{l})$ - - -	60493,42	13 34 44
$\overset{(6)}{m} = \overset{(1)}{m} + Q(\sin^2.\overset{(6)}{l} - \sin^2.\overset{(1)}{l})$ - - -	60498,13	14 34 44
$\overset{(7)}{m} = \overset{(1)}{m} + Q(\sin^2.\overset{(7)}{l} - \sin^2.\overset{(1)}{l})$ - - -	60503,13	15 34 44
$\overset{(8)}{m} = \overset{(1)}{m} + Q(\sin^2.\overset{(8)}{l} - \sin^2.\overset{(1)}{l})$ - - -	60508,44	16 34 44
$\overset{(9)}{m} = \overset{(1)}{m} + Q(\sin^2.\overset{(9)}{l} - \sin^2.\overset{(1)}{l})$ - - -	60514,03	17 34 44
Their sum	544450,10 = A	

With respect to the dimensions of the earth, and the length of the quadrantal arc of the elliptic meridian, let $^{(1)}l = 13°34'44''$; $m' = 60493{,}42$ fathoms, $e = \frac{1}{310{,}31} = {,}0032226$; then $3\,e = {,}0096678$; $3\,e \cdot \sin^2.{}^{(1)}l = {,}0005329$. Then if m be the degree on the meridian at the equator, where sin. l is o, $m = \frac{m'}{1 + 3\,e \cdot \sin^2.{}^{(1)}l} = \frac{60493{,}42}{1{,}0005329} = 60461{,}2$ fathoms, and therefore $60461{,}2 \cdot \frac{1 + 3\,e}{1 + e} = 60850{,}17$ fathoms, the measure of the degree on the equatorial circle (see the different equations in the Philosophical Transactions for 1818, Part II.)

Put $A = 57°$, &c. the arc equal rad: then $A\,m = 3486457{,}9$ and therefore $2 \times 3486457{,}9 = 6972915{,}8$ fathoms for the diameter of the equatorial circle; equal the major axis of the elliptical meridian, which call a. Then if the minor axis be designated by b, we have $b = (1 - e)\,a = 6950442$ fathoms for the polar axis of the spheroid, supposing it to be an ellipsoid. But $3{,}14159$, &c. $\times\, 6972916 = 21906074$ the circumference of the circumscribing circle. Then if $d = 1 - \frac{b^2}{a^2} = {,}0064355$; we have $1 : 1 - \left(\frac{d}{2^2} + \frac{3\,d^2}{2^2.4^2}\right) :: 21906074 : 21871024$, the length of the elliptic meridian. Hence $\frac{21871024}{4} = 5467756$ is the length of the quadrantal arc, which, reduced to inches, and multiplied by 10,000000 we get 39,3677 inches for the metre at the temperature of 62°, which falls short of the French metre by ,0032 inches, when reduced to the same temperature.

This conclusion is very satisfactory, and I hope that equal success will attend my operations to the northward. I have already measured another section, which extends to latitude

21° 6′, having just returned from finishing it; and when all the necessary calculations and corrections are made, I shall draw out an account of the whole, and forward it to the Royal Society at a future period. The celestial arc has been determined by seven stars, but there are many now out of my reach, which I observed in the beginning.

It may be satisfactory to the mathematicians in Europe to know, that I am now advancing through Hindoostan; and, from what I can learn from the different publick authorities, I do not apprehend any difficulty. They are all inviting in their letters, and all seem desirous that I should go through their respective districts. If my present arc be continued direct, it will pass through Bopaul, and near Seronje, where I shall have again to observe the stars and measure a base; and if Scindiah's country be in a quiet state, my meridian will pass near Gualior, his capital; and my sixth section will terminate near Agra, on the Jumna. I have made up my mind to execute all this if I live, and continue to have that flow of health and spirits which have hitherto attended me. The results of such an extensive measurement must be interesting to scientific men; and I shall exert my endeavours in doing justice to the work, and in giving a faithful account of the operations.

V. *On the changes which have taken place in the declination of some of the principal fixed Stars.* *By* John Pond, *Esq. Astronomer Royal, F. R. S.*

Read April 18, 1822.

The mural circle having in September last been put into complete repair, and declared by Mr. Troughton to be in as perfect a state as when first erected, I resumed my examination of the principal fixed Stars which form the Greenwich Catalogue. In the course of a very short time, I found that several anomalies, which had previously given me much perplexity, still subsisted: some of these were of such a nature as to lead to a suspicion that a change might possibly have taken place in the figure of the instrument; on the other hand, there were circumstances, that strongly militated against such a supposition.

Several of the stars in which the supposed discordance appeared the greatest, passed over almost the same divisions with others, in which no such discordance could be perceived. Moreover, in examining these discordances in different points of view (that is, both with respect to their right ascensions and polar distances) I fancied I perceived something like a general law, that was quite incompatible with any possible hypothesis of error in the instrument.

On a point of this importance, I clearly saw the necessity of devising some new method of observation which might

decide with certainty, that which otherwise would become an endless subject of doubt and conjecture.

I had often attempted to observe the altitudes of stars by means of an artificial horizon of quicksilver, or other fluid, but had abandoned the attempt from the difficulty of protecting it from the wind, and from the number of observations I lost in fruitless experiments. To this method I had again recourse; and by means of wooden boxes of different sizes and figures, according to the different altitudes of the stars, I have sufficiently accomplished my purpose. A very few observations were sufficient to convince me that the instrument was in every respect perfect, and that I might repose the greatest confidence in every result it gave.

Several stars, and particularly those most discordant, I have observed by this new method, and find their places, without any exception, to agree within a fraction of a second, with those determined by direct measurement from the pole.

Presuming that the observations* which accompany this paper will remove every shadow of a doubt as to the accuracy of the instrument, I shall now proceed to state, in as few words as possible, the nature of the changes which appear to me to have taken place since the year 1812.

If Bradley's catalogue of stars for the year 1756, be compared with the Greenwich catalogue for 1813, it will be possible to deduce the annual variation for each star for the mean period, or for the year 1784, on the supposition of uniformity in the proper motion of each star; then allowing for the change of precession for each star, a catalogue may

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* Vide Appendix.

be computed for any distant period ; as for example, the present year 1822. Suppose such a catalogue computed, which I have named a predicted catalogue ; then, if this be compared with the observed catalogue for the same year, the following differences will be found to subsist between them.

The general tendency of all the stars will be to appear to the south of their predicted places, and this tendency seems to be greater in southern than in northern stars ; if any star be found north of its predicted place, it will always be a star north of the zenith, and the quantity of its motion extremely small. There may be observed a much greater tendency to southern motion in some parts of the heavens than in opposite, or distant parts as to right ascension, and in much the greater portion of the heavens the southern motion seems to prevail. A southern star, as Sirius, situated in that part of the heavens most favourable for southern motion, will be found more to the south of its predicted place than Antares, situated in the part least favourable for southern motion, though it is itself more southward.

Several stars have moved more from their predicted places than other neighbouring stars ; when this happens, the motion is always southward ; I have yet met with no exception to this rule ; not a single star can be found having an *extra* tendency to northern motion ; and indeed the northern motion in any star is so very small, that it would never have excited attention.

A very great deviation will be found in three very bright stars, Capella, Procyon, and Sirius : the proper motion of each of these is southward ; it therefore follows that these proper motions are accelerated. The proper motion of Arc-

turus is very great, and likewise southward. It is situated in that part of the heavens where the southern tendency is least discernible, and is nearly quiescent; its proper motion in polar distance may therefore be considered as uniform. There is a circumstance that deserves notice, though it may be merely accidental: the stars in the Greenwich catalogue, whose proper motions are south, nearly equal in number those that are north, yet the *quantity* of southern proper motion exceeds the northern in the proportion of four to one.

I shall at present offer no conjecture on the cause of these deviations, but endeavour, by continued observations, more accurately to ascertain the law which they follow. Should the weather prove favourable for observation, I hope before the Society separate for the summer, to be able to give greater accuracy to the numbers here subjoined. Indeed I should not have made so early a communication on the subject, but as the Greenwich observations of 1820 are about to be published, they might without this explanation have appeared erroneous; for I find that during that year the instrument was rather defective from general unsteadiness, than from any perceptible deviation of the telescope. It was not till after the month of February, 1821, that the instrument got completely out of repair. It must however be admitted, that the observations of that year ought not to be employed in the determination of such small quantities as form the subject of the present communication.

Horizontal point of the Circle as found by different Stars observed by direct vision and reflection, from 11*th to* 23*rd March,* 1822.

	°	′	″
h Urs. Maj.	123	30	29,55
v			28,95
m			29,75
β			29,45
α			29,50
o			29,05
Castor			29,86
Capella			29,55
Pollux			29,95
β Aurigæ			29,35
Mean of 10			29,54
Sirius			29,47

There being no perceptible difference in the results obtained near the zenith and near the horizon, it may be concluded that the instrument has no deviation, either from flexion of the telescope or change of figure.

VI. *Appendix to the preceding Paper on the changes which appear to have taken place in the declination of some of the fixed Stars.*

By J. Pond, *Esq. Astronomer Royal, F. R. S.*

Read November 14, 1822.

THE observations which have been made during the last summer, confirm in a very decided manner the results which formed the subject of my last communication; in which I laid before the Society the nature of the differences that exist between the computed places of the principal Stars of the Greenwich Catalogue, and those deduced from actual observation. It is not my present intention to offer any explanation of the cause of these phenomena, although many obvious conjectures present themselves, the value of which it will require perhaps many years to determine. It is now my principal object to consider the force of that explanation of the differences in question, which will most readily occur to every astronomer, namely, that the whole may arise either from error committed by the observer, or from defect in the instruments of observation: this objection being the more weighty from the circumstance, that the observations of three distant periods are employed, and that an error in those of either period (but particularly of the two latter) would materially affect the result now under consideration.

I believe that every person, in proportion to his experience in the use of astronomical instruments, (even of the most unexceptionable construction), will be cautious in admitting

the accuracy of any results, with whatever care the observations may have been made, which appear to militate against any received theory of astronomy; and I shall have occasion myself to show, from the great discordances between instruments of the highest reputation, that this distrust is but too well founded. More particularly ought our suspicion to be excited, when such anomalies are found to exist, as bear some direct proportion to the zenith distances of the stars observed. In all such cases we should never hesitate, I think, to ascribe the anomalies to defective observation. If therefore in the present instance, any part of the discordances in question can be shown to depend on polar or zenith distances, I shall willingly admit, as to such part of them at least, that they are no otherwise of importance, than as affording data for leading to the detection of some hitherto undiscovered errors. The anomalies, however, that have led me on to this enquiry, and to which alone I attach any importance, are found to depend rather on the right ascensions, than on the declinations of the stars. Accordingly I found, while collecting observations to form a catalogue for the present period, that I could more nearly predict the deviation of a star from its computed place, by knowing its right ascension, than its declination. Now it is not easy to conceive in what way the error of an instrument for measuring declination, fixed in the meridian, can be occasioned by any circumstance depending on the right ascension of a star to be observed.

The general nature of the deviation of the stars from their computed places will be best understood from the annexed tables; in one of which the principal Stars of the Greenwich Catalogue are arranged according to north polar distance, and in the other, in the order of their right ascensions.

From these tables it will appear, according to my statement in the former part of this paper, that the general tendency of the deviation is towards the south: that in about one-third part of the heavens in right ascension this southern tendency is very inconsiderable, and would hardly have excited attention: for in this part, stars between the zenith and the pole, appear a very small quantity to the northward; whereas in the remaining, and most considerable portion of the heavens, every star appears to be a considerable quantity to the south of its computed place; and with few exceptions, the more southward stars have a greater tendency to deviation than the northern ones.

If we select from the preceding tables, those stars which were least frequently observed, at one or all of the three periods, we shall find that they all tend to confirm the foregoing general results; though they must be regarded as doing so, rather by their united effect, than by their weight of evidence when considered singly. Stars that have been but seldom observed, give results considerably affected by accidental error of observation; which error is quite of a different nature from that produced by permanent defect in the instrument, and which repetition of observation has no tendency to remove.

If the deviations of those stars that have been imperfectly observed, were attributable either to error of observation, or defect in the instruments, the deviation would either follow no law at all, or some law depending upon zenith distance: but the facts we have seen to be at variance with either of these hypotheses. Not however to rest satisfied with these considerations drawn from the general tendency of all the stars

without exception, let us select some striking examples of deviation, in particular groups of stars, on which we might be satisfied to rest the issue of this question. Of these groups I have marked *five*, in the table of stars arranged according to north-polar distance, each of which we will take the pains to consider more attentively.

1. There are six stars in my Catalogue north of γ Draconis, of which three are found to the north, and three to the south of their computed places. These inequalities may appear at first sight to be wholly accidental; but if we pay attention to the right ascension, we shall find that the three which appear to the northward, are situated in that part of the heavens as to right ascension where the southern deviation is the least perceptible, and that the three which appear to the southward, are in that part as to right ascension where the southern deviation is the greatest. But of these six stars there are two, α Cassiopeiæ, and γ Ursæ Majoris, which deserve farther consideration. These two stars are within less than one degree of each other in polar distance, and consequently pass over the meridian at nearly the same altitude. The observations of Bradley on the stars north of the zenith are not so numerous as could be wished; but each of the two stars in question was observed by him about five times towards the year 1753; that is 60 years from the date of my catalogue of 1813. I have carefully recomputed the predicted places of these stars, and I find α Cassiopeiæ not less than 1″,5 to the south of its predicted place, and γ Ursæ Majoris half a second to the north. Now I am quite at a loss to conceive how this difference in so small an arc can arise from error of observation, and I can only attribute it to that cause, whatever it may be, which

seems so generally to depend not on the polar distance, but on the right ascension of the star.

2. The second group which I shall consider, contains the stars α Arietis, Arcturus, and Aldebaran, comprehended within an arc of about six degrees and a half. Of these three, Arcturus alone has yet been observed by reflection; but from the present very perfect state of the Greenwich circle, which the method of reflection has enabled me to ascertain, it cannot be doubted that the places of the two other stars are well determined.* In Arcturus the southern deviation is nearly insensible, but in the two other stars it is very considerable, being in each not less than 1″,5. Now these three stars, but particularly the two latter, are among those that have been most assiduously observed by Bradley and myself, at each of the three periods. Let us suppose then, if it be possible, that the whole of these deviations arise from error of observation; or in other words, that no systematic deviation has really taken place in the stars, but that their proper motions are uniform. Then we must admit that the mural quadrant and the mural circle have at each period given the polar distance of Arcturus correct, or at least subject to the same constant error; and as this star has been observed at each period, at all times of the day, and at all seasons of the year, the observations may be considered as perfectly exempt from accidental error. It will I believe be readily conceded that both instruments are so far perfect, that if the error be either nothing, or a given quantity at one point of the arc, the errors must be very nearly indeed the same within a moderate distance, as within 15 degrees, for instance, of that point. Upon this supposition, how can we possibly reconcile the great errors that must

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* This has been confirmed by subsequent observation.

have been committed in stars, adjacent as to polar distance, but of opposite right ascensions? I do not wish to press these remarks, in order to obtain greater confidence than they deserve, for observations which can never be regarded with too much suspicion; but the arguments I have used, appear to me to follow logically from the data before us, and strongly to indicate the probability that some cause purely astronomical has, at least, some share in producing these unexpected deviations.

3. The third group, α Herculis, α Pegasi, and Regulus is still more remarkable, being comprehended within two degrees of declination, and two of the stars, α Herculis and α Pegasi* being within half a degree of each other. In this group α Pegasi is at least 3″ south of its predicted place, whereas the other two stars have not deviated much more than 0″,5 to the south.

4. α Orionis, α Serpentis, and Procyon, furnish an example equally striking, they being within less than 2° of declination from each other; α Serpentis is exactly in its predicted place, while α Orionis and Procyon are each of them at least 2″ to the south.

5. Rigel, Spica Virginis, and Sirius, are not contained within so short an arc as the former groups, nor are their places so well determined, on account of their proximity to the horizon; but they afford another instance of the inequality of southern deviation, in stars having nearly the same polar distance, but opposite right ascensions.

But leaving the considerations suggested by these groups of stars, let us examine more minutely the different hypotheses that may be formed on the supposition, that the whole

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* The lunar nutation of α Pegasi was nearly a minimum at each period.

of these deviations depends on error of observation caused by some defect in the instruments employed: this investigation becomes the more necessary, as it does not appear that Dr. BRINKLEY, with his instrument at Dublin, has met with similar discordances. Admitting the accuracy of the observations of BRADLEY to form the ground-work of this enquiry, there are then two distinct hypotheses, that may be formed by those, who are inclined to maintain, that the proper motions of the stars are uniform; and that the discordances in question have their source, not in any astronomical cause, but in some erroneous system of observation. Of the observations from which the catalogues of 1813 and of the present year have been computed, we may suppose the one or the other to be erroneous. Let us consider the consequences of each hypothesis.

Let us first suppose the error to be in the observations of 1813. Then the observations of 1756 and 1822 being supposed perfect, a catalogue for the year 1813 may be computed by interpolation; such a catalogue is annexed, and this, (assumed to be correct,) compared with the observed catalogue of 1813, will show the errors of observations at that period. On this assumption the Greenwich circle must, in 1813, have been in a very defective state; and admitting the instrument to be now perfect, this can be only attributed to the insufficiency of the braces which then connected the telescope to the circle; for this is the only difference between the instrument in its former and in its present state. The natural tendency of any such defect would be, I think, continually to increase, and to give results every year more and more distant from the truth: but this is contrary to the known history of the Greenwich observations, which I

have found gradually for some time past approaching to those results which are obtained at the present day, and which, according to our present hypothesis are supposed to be nearly perfect. If the catalogue of 1813 were really so erroneous, as our present hypothesis would compel us to regard it, then it would appear that Dr. Brinkley's catalogue for the same period must have been still more erroneous, as may be seen by inspection of the annexed tables. Now admitting for a moment that there were at that time certain imperfections in the Greenwich and Dublin instruments, no person will believe them to have been so imperfect as our present hypothesis would tend to represent them.

Let us now examine the second hypothesis, which presumes the catalogue of 1813 to have been perfect, and consider what confidence is due to the Greenwich observations of the present day. This investigation is to be regarded as important, not merely with a view to the discussion of the nature of the discordances in question, but also from the circumstance, that instruments of well-known celebrity are represented as giving very different results; for which reason I shall be excused for entering into considerable details on this particular question. As the principal reliance I place on the accuracy of the present catalogue, and on the superiority of the Greenwich circle over all other instruments, with the history of which I am acquainted, is derived from the coincidence of the results obtained by the two independent methods; the one of direct measurement of polar distance, the other of observing the angular distance of the direct and reflected image of the stars, it becomes of some importance to consider in what way this coincidence is a proof of the accuracy of either. The source

of error the most to be dreaded in every instrument whatever, quadrant or circle, is that which will be caused by the flexure of the materials of which the instrument is made. It is impossible in theory that any instrument can be wholly free from this defect. In the Greenwich circle the number of microscopes placed round its circumference have an obvious tendency to diminish this error, though they cannot annihilate it; but they have no tendency whatever to diminish the error arising from the flexure of the telescope attached to the circle.

The effect of flexure in any circle will be, in the first instance, to give an erroneous distance from the pole to the zenith: in instruments that turn in azimuth, of the usual construction, the error thus occasioned will be applied to every star under the form of co-latitude, and a star south of the zenith, will be moreover affected by the probably opposite flexure due to that point of the instrument on which the star is observed. This in stars near the equator, or a little to the northward of it, will in our latitude give an error in polar distance, amounting to about double the error committed in determining the co-latitude. On the contrary, the polar distances of stars north of the zenith, being affected only by the difference of two flexures, will be more accurately determined as they approach nearer to the pole, where the errors will wholly vanish. Now, though in the usual mode of employing the Greenwich circle, viz. in measuring directly polar distance, the co-latitude does not become an object of enquiry, yet any flexure of the circle will produce a system of errors of the same nature as those above pointed out. In instruments, like that of Dublin, which turn in azimuth, and with which the observer has to find the place of all the stars by measur-

ing the double of their zenith distances, if he does not find the same zenith point with different stars (provided the instrument be well divided) he may be sure that flexure takes place; but he cannot infer the converse, that flexure does not take place, from his obtaining with all the stars the same error in the line of collimation. For if the flexure be the same on both sides of the zenith, a supposition by no means improbable, the observer will then have no indication of flexure by the usual method of determining the error of collimation by stars of different altitudes. Let us suppose that, with an instrument liable to flexure, it is required to measure by both methods the meridional distance of any two stars. The angular distance of the direct images will (as we have already seen) be affected by the difference, or by the sum of two flexures, according as the stars are placed on the same, or on opposite sides of the zenith. In viewing the reflected images, the instrument receiving two new positions, will be subject to two new flexures, by the sum or difference of which (as it may happen) the angular distance of the reflected images will be affected.

The most probable supposition to be made concerning the flexures is, that at equal inclinations with the horizon, above and below it, they will be the same nearly both in direction and degree, and therefore that the two images below the horizon will approach by nearly the same quantity that the direct images receded, or vice versâ. With an instrument therefore having such a system of flexures, the double altitude of each star will be correctly ascertained; but stars of different altitudes will give different determinations of the horizontal point. From observations thus obtained, a near approxima-

tion to the true angular distance might be inferred, by taking a mean between the distances of the direct and of the reflected images. The least probable supposition concerning the flexures is, that at equal inclinations above and below the horizon, they will be equal, but in opposite directions ; the consequence of which would be, that the direct and reflected images would approach to or recede from one another by the same quantity : the double altitudes of each star would be incorrectly given, but every star would give the same determination of the horizontal point. To suppose however the existence of such a system of flexures, would be to suppose that gravity produced the same change of form in the instrument, as if its direction were inverted ; and since the horizontal line is that, at which according to the supposed system a contrary flexure will take place, the flexure at or near the horizon should be zero, where, however, according to the known laws of mechanics it ought to be the greatest. Such a system therefore must be considered as mechanically next to impossible.

If then an instrument give the angular distances both by reflection and by direct vision the same, and the same determination of the horizontal line from stars of whatever altitude, there are then only two hypotheses that can be formed respecting such an instrument ; either that the flexures are insensible, or that they are such as are absolutely inconsistent with the laws of mechanics. Hence I conclude that the coincidence of the results by direct vision and by reflection, and the uniform determination of the horizontal point, will be the strongest proof of the non-flexure of the instrument, and of the accuracy of both results.*

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* I must also notice that the method by reflection possesses, in common with

In illustration of the whole of the preceding observations let us examine two catalogues, those of Dr. Brinkley, and Mr. Bessel, which have lately much excited the attention of astronomers. It is obvious, by merely inspecting these catalogues, a comparison of which with the Greenwich catalogue I here subjoin, that one, or both, of the instruments used by these astronomers must be erroneous; and it seems to me, that the source of error is the very flexure, the nature and effects of which we have been considering. For if we attend to the differences between these two catalogues, we shall find, that the six stars near the equator differ 5″ from one another, whereas the stars near the zenith do not differ above 2″,5. In which direction flexure will effect the zenith distances, is a matter quite accidental, depending on the unequal elevation or depression of the object-end or eye-end of the telescope, in consequence of the unequal strength of the materials. If we suppose error to exist in each of the catalogues, this cause must have had an opposite influence in the two cases: if we compare the Greenwich observations with those of Dr. Brinkley, we shall arrive at the same conclusion; namely, that the differences must be caused by flexure in one or both of the instruments; since here also we find that the stars in the neighbourhood of the zenith are affected by only half the difference in polar distance, that is observed in the stars near the equator; and the same conclusions may be drawn from comparing the Greenwich observations with those of Mr. Bessel. The polar distances of all the stars in Mr. Bessel's catalogue exceed the

instruments turning in azimuth, the advantage of measuring the double of the required angle.

polar distances given in the Greenwich catalogue; while those of all the stars in Dr. BRINKLEY's catalogue as regularly fall short of my determinations. It is not from the casual circumstance of my results being nearly a mean between the results of those two astronomers, that I intend to claim a superior weight of authority for my own; for were this the only ground for preference, I should regard the question as yet undetermined, and should think it my duty to recommend the providing of new and more powerful instruments for ascertaining the truth. But it appears to me that from the observations by reflection, which I have lately made, and from their agreement with my observations by direct vision, that I am entitled to determine the share of error to which each of these two catalogues is liable; not only from the general superiority of the Greenwich circle, which I consider to have been thus proved, but from this peculiar circumstance, that whereas in the two catalogues of Mr. BESSEL and Dr. BRINKLEY, the errors cannot fail to be the greatest in stars near the horizon; by my method of reflection those stars, which are nearest the horizon, must be determined the most correctly, from their double altitudes being measured on the smallest arc.

In stars near the equator the catalogue of Mr. BESSEL differs from that of Dr. BRINKLEY five seconds; and from the preceding considerations, I think we may venture to conclude that Mr. BESSEL's polar distances are too great by about three seconds, and Dr. BRINKLEY's too small by about two: and since my catalogue differs from the two former from the zenith to equator in very nearly the same proportion, there can be no reason to doubt that their errors throughout are divided in nearly the same ratio.

With regard to the catalogue for the present period, which accompanies this paper, I beg to state that I consider it only as a very near approximation to the truth, and requiring at least another year's observations, to render it of equal value with that of 1813, which is the result of two years observations with six microscopes, and in four positions of the telescope.

I am persuaded that the more this subject is considered, the more distinctly it will appear, that if any doubt can be entertained, founded on any circumstance arising out of the Dublin observations, that doubt must relate, not to the accuracy of former catalogues, but to the present position of the stars; since it is with respect to their *present* position that the two instruments are really at variance. This circumstance is very fortunate, as time may confirm the present, or suggest some more satisfactory method of investigation, if what I have now advanced be not thought sufficient for the purpose.

VII. *On the parallax of α Lyræ.* *By* John Pond, *Esq. Astronomer Royal, F. R. S.*

Read November 14, 1822.

My former experiments with a fixed telescope upon α Cygni have always appeared to me so decisive, as to render hopeless any farther attempt to discover its parallax; but respecting that of α Lyræ, my observations with the mural circle were not equally satisfactory; for among the observations of this star we may find occasional discordances that admit of being interpreted in favour of parallax. And although I have been inclined myself to attribute these irregularities to other causes, yet their existence made it desirable to institute new experiments. The method with a fixed telescope, which I had contrived for α Cygni, could not here, I found, be applied successfully; there being no star of nearly the same altitude but opposite in right ascension sufficiently bright to be observed throughout the year, a circumstance quite essential to that mode of observation. I have employed therefore the mural circle to investigate, 1st, the difference of parallax between γ Draconis and α Lyræ: 2dly, the absolute parallax of the latter star; the Dublin observations indicating, it may be remembered, that the parallax of γ Draconis is insensible, but that of α Lyræ a very perceptible quantity. The processes employed in these two investigations being very different, I shall consider each of them separately.

On the difference of parallax between γ Draconis and α Lyræ.

It is impossible to conceive a more simple process than that of determining with the mural circle the difference of polar distance between these stars. From their proximity in right ascension, the operation is the same as that of measuring the angular distance of two terrestrial objects, about 12° asunder, with a theodolite surrounded by six microscopes: for the mural circle, in principle, exactly resembles a vertical theodolite; with this difference, that its microscopes, instead of being placed on a frame-work of brass, are securely fixed on a stone pier. Now I find that the angular distance thus measured in winter does not differ one-tenth of a second from the same angular distance measured in summer; and therefore, that the difference of parallax between the two stars is absolutely a quantity too small to be measured. In this investigation, it is to be considered that any constant error in the determination of the absolute polar distances has nothing to do with the question, it being the difference only of those distances at opposite seasons that is required. To render all errors throughout the whole course of observation as constant as possible, the telescope remained fixed to the same part of the limb of the instrument, and the utmost pains were taken to reduce the temperature in the Observatory to that of the outer air; the difference throughout the year not exceeding one degree. The winter of 1821-1822 was extremely favourable for astronomical observation; there were an unusual number of fine nights, and the weather was so mild and uniform, that we were enabled to equalize the temperature, so as to make it of no importance whether the observations were

computed by the outer or inner thermometer; and it is to this circumstance, in a great measure, that I attribute the perfect coincidence between the observations at different seasons.

It has been objected, however, that perhaps some unexpected effect of temperature deranges the instrument by the exact quantity of the difference of parallax attributed to these stars by Dr. Brinkley; if we suppose a derangement from temperature so considerable as to give a sensible error, even after being diminished by the effect of six microscopes, we should expect the error to be much greater when the experiment is tried with two microscopes only; for to suppose the contrary, would be to deny the tendency of six microscopes to correct the errors of two. Now I find the same difference of polar distance whether I employ two microscopes or six; temperature, therefore, cannot materially have vitiated the results by causing derangement in the form of the instrument.

In the whole of the above process I do not see one objectionable point, and if called upon to invent an instrument for this particular experiment, I could not devise one more perfect in principle than the mural circle.

Whoever will compare the above simple process with the more complicated one necessarily employed in using an instrument with two microscopes, turning freely in azimuth, will not hesitate, I think, in deciding upon which of the two instruments temperature is likely to produce the greatest error.

On the absolute parallax of α Lyræ.

The preceding observations only indicate that γ Draconis

and α Lyræ have the same parallax, or that their difference of parallax is zero; but they have no tendency to show what is the actual magnitude of the parallax that the two stars have in common. If indeed we admit it to be proved, by the observations of BRADLEY, and the more recent ones of Dr. BRINKLEY, that the parallax of γ Draconis is insensible, we may then infer from the observed difference what is the parallax of the other star. But the method of investigation that we are now about to consider, does not depend on such an admission.

Having successfully adopted the method of observing by reflection, I was desirous of employing it in a series of observations upon α Lyræ, with a view to determine this question. This series began on the 1st of July, 1822, and has been continued to the present time.* Although this period embraces only half the interval in which the greatest change or double parallax is affected, a circumstance which at first may appear very disadvantageous, yet that is more than compensated, in my opinion, by the number of observations, and by a uniformity of temperature, such as never can be expected in the extreme seasons of winter and summer.

In observations of this nature the effects of temperature upon the instrument itself, and the uncertain refractions of the ray of light when brought into the lower part of the room, may produce errors of no inconsiderable magnitude, with reference to a question of so much nicety as the present.

I can show however in the present as in the former process, that no error from temperature, affecting the instrument, has

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* Since the date of this paper, the observations have been continued throughout the winter, and the results will be found in the subjoined Table.

introduced itself into this series of observations; for I obtain the same result from the readings with two microscopes as from those made with six.

In the case of two microscopes, the angular distance is measured upon two arcs only. Now it cannot be for a moment contended that an error from temperature, so great as not to be corrected by six microscopes, will not be much exaggerated by employing only two. The errors then, if any, must arise from the effects of temperature on refraction, and not from the changes it occasions in the instrument. But from the season which I have chosen for this investigation, and from the care that has been taken to equalize the temperature, the errors arising from the latter cause must be almost insensible. My observations, thus conducted, indicate in the most decided manner, that the parallax of α Lyræ cannot exceed a very small fraction of a second. The advantages and disadvantages of the Dublin and Greenwich methods are in this process much more nearly balanced than in the former. The Dublin instrument has the great advantage of determining the zenith distance in the course of a few minutes, whereas at Greenwich twenty-four hours at least, and frequently several days elapse, before a complete observation of the double altitude can be obtained by the method of reflection. This disadvantage attending the Greenwich method could only be remedied by employing two mural circles for observing a star on the same night, both by direct vision and by reflection.

I have now to consider that argument on which the greatest reliance in favour of parallax has been placed, namely, that founded on the actual determination of the solar equation from the observations made with the Dublin instrument.

This argument may, I think, be thus stated. By a series

of observations made with a given instrument two equations have been disengaged, previously considered as unknown in amount, but known only as to the law of their variation. Of these one is much smaller than the other. Hence it is inferred, that as the instrument has faithfully disengaged the smaller equation (respecting which there is no dispute), it must be admitted with equal fidelity to have disengaged the larger, which might be supposed the easier operation of the two. This reasoning is strictly logical, as proving the disengagement of two equations, but it by no means proves the larger equation to be caused by parallax. The larger equation here to be disengaged is after all so small, that it is impossible, in different points of its period, to show that the law assumed coincides with observation; it is only a rude agreement at the points of the greatest and least variation that can be demonstrated. The disengagement of the larger equation only proves therefore the existence of some regularly recurring cause, acting with greatest effect at the extreme seasons.

The reason, I conceive, why Dr. Brinkley does not find parallax in γ Draconis is, that with respect to the zenith point, his instrument, like every one of a similar construction, is a perfect instrument. No portion of the arc is employed, nor can temperature here occasion any errors by its changes. As the star to be examined recedes from the zenith, the instrument becomes less and less perfect; and he finds a small parallax in α Cygni, a larger in α Lyræ, and oftentimes a still larger in stars more remote from the zenith. An additional reason for suspecting that the discordances observed arise from temperature is this: the greatest supposed parallax is found in those stars whose maximum and minimum of parallax would fall in the ex-

treme seasons, and it is not at all improbable that irregular refraction, arising from the unequal state of the temperature within and without the Observatory, may have had a considerable share in occasioning the Dublin discordances, combined, perhaps, with the effect of the changes of temperature upon the instrument itself. It is a circumstance not hitherto sufficiently noticed by astronomers, that there are many cases where the smallest disturbing cause will produce an error quadruple of its own amount; and consequently, that the greatest error to which we are liable from such a cause at any one observation will be only one-fourth of the difference that we can detect between the most discordant of them. Of such a nature are those disturbances which, like refraction for instance, introduce errors, both positive and negative, into the determination of either extremity of the arc that measures the distance between two stars.

By a singular combination of circumstances, not probable certainly when considered *a priori*, but by no means impossible, the variation caused by change of temperature may follow an annual law so little differing from that of parallax, as to bring out the assumed parallax, and to leave the solar nutation disengaged.

Notwithstanding the importance of these investigations to the history of astronomy, and to our forming a correct notion of the system of the universe, yet our decision ultimately turns upon so very small a quantity, that our having reduced the enquiry to these narrow limits, rather tends to show the perfection of each instrument than the defect of either.

On former occasions I considered the question of parallax in the particular case of α Lyræ as undecided, and as perfectly open to future investigation; but the observations of the present year

have produced, on my mind, a conviction approaching to moral certainty. The history of annual parallax appears to me to be this: in proportion as instruments have been imperfect in their construction, they have misled observers into the belief of the existence of sensible parallax. This has happened in Italy to astronomers of the very first reputation. The Dublin instrument is superior to any of a similar construction on the Continent; and accordingly, it shows a much less parallax than the Italian astronomers imagined they had detected. Conceiving that I have established, beyond a doubt, that the Greenwich instrument approaches still nearer to perfection, I can come to no other conclusion than that this is the reason why it discovers no parallax at all.

TABLE I.

	Names of Stars.	Predicted N. P. D. 1823.	Observed N. P. D. 1823.	Diff. South.	
		° ′ ″	° ′ ″	″	
1	Polaris				
2	β Urs. Min.	15 7 16,38	15 7 15,7	— 0,7	
3	β Cephei	20 12 53,78	20 12 54,1	+ 0,3	
4	α Urs. Maj.	27 17 44,16	27 17 43,6	— 0,6	
5	α Cephei	28 9 42,20	28 9 42,8	+ 0,6	
6	α Cassiop.	34 26 4,23	34 26 5,8	+ 1,6	1
7	γ Urs. Maj.	35 19 15,18	35 19 14,8	— 0,4	
8	γ Draconis	38 29 10,31	38 29 10,5	+ 0,2	
9	η Urs. Maj.	39 47 59,55	39 47 59,6	— 0,1	
10	α Persei	40 46 38,34	40 46 39,0	+ 0,7	
11	Capella	44 11 35,11	44 11 36,9	+ 1,8	
12	α Cygni	45 20 50,80	45 20 52,4	+ 1,6	
13	α Lyræ	51 22 30,37	51 22 31,2	+ 0,9	
14	Castor	57 43 58,08	57 43 59,1	+ 1,0	
15	Pollux	61 33 16,76	61 33 17,0	+ 0,3	
16	β Tauri	61 33 5,74	61 33 6,7	+ 1,0	
17	α Androm.	61 53 10,75	61 53 12,4	+ 1,7	
18	α Cor. Bor.	62 41 0,07	62 41 0,6	+ 0,5	
19	α Arietis	67 22 42,60	67 22 44,4	+ 1,8	2
20	Arcturus	69 53 28,83	69 53 29,2	+ 0,4	
21	Aldebaran	73 51 16,17	73 51 17,7	+ 1,5	
22	β Leonis	74 26 17,73	74 26 18,1	+ 0,4	
23	α Herculis	75 23 59,70	75 24 0,0	+ 0,3	3
24	α Pegasi	75 44 38,52	75 44 41,7	+ 3,2	
25	Regulus	77 10 15,00	77 10 15,4	+ 0,4	
26	α Ophiuchi	77 18 9,74	77 18 10,6	+ 0,9	
27	α Aquilæ	81 35 28,31	81 35 29,4	+ 1,1	
28	α Orionis	82 38 2,11	82 38 4,2	+ 2,1	4
29	α Serpentis	84 0 36,52	84 0 36,6	+ 0,1	
30	Procyon	84 19 40,75	84 19 43,2	+ 2,5	
31	α Ceti	86 36 34,86	86 36 36,9	+ 2,1	
32	α Aquarii	91 10 28,85	91 10 31,3	+ 2,5	
33	α Hydræ	97 53 43,20	97 53 44,5	+ 1,3	
34	Rigel	98 24 46,44	98 24 48,5	+ 2,0	5
35	Spica Virg.	100 14 0,73	100 14 0,7	0,0	
36	Sirius	106 28 45,35	106 28 48,9	+ 3,5	
37	Antares	116 1 42,50	116 1 44,1	+ 1,6	

TABLE II.

	Names of Stars.	Predicted N. P. D. 1823.			Observed N. P. D. 1823.			Diff. South.
		°	′	″	°	′	″	″
1	α Cassiopeiæ	34	26	4,23	34	26	5,8	+ 1,6
2	Polaris							
3	α Arietis	67	22	42,60	67	22	44,4	+ 1,8
4	α Ceti	86	36	34,86	86	36	36,9	+ 2,0
5	α Persei	40	46	38,34	40	46	39,0	+ 0,7
6	Aldebaran	73	51	16,17	73	51	17,7	+ 1,5
7	Capella	44	11	35,11	44	11	36,9	+ 1,8
8	Rigel	98	24	46,44	98	24	48,5	+ 2,0
9	β Tauri	61	33	5,74	61	33	6,5	+ 0,8
10	α Orionis	82	38	2,11	82	38	4,2	+ 2,1
11	Sirius	106	28	45,35	106	28	48,9	+ 3,5
12	Castor	57	43	58,08	57	43	59,1	+ 1,0
13	Procyon	84	19	40,75	84	19	43,2	+ 2,5
14	Pollux	61	33	16,76	61	33	17,0	+ 0,2
15	α Hydræ	97	53	43,20	97	53	44,5	+ 1,3
16	Regulus	77	10	15,00	77	10	15,6	+ 0,4
17	α Urs. Maj.	27	17	44,16	27	17	43,4	— 0,6
18	β Leonis	74	26	17,73	74	26	18,1	+ 0,4
19	γ Urs. Maj.	35	19	15,18	35	19	14,4	— 0,4
20	Spica Virg.	100	14	0,73	100	14	0,7	0,0
21	η Urs. Maj.	39	47	59,60	39	47	59,5	— 0,1
22	Arcturus	69	53	28,83	69	53	29,2	+ 0,4
23	β Urs Min.	15	7	16,38	15	7	15,6	— 0,8
24	α Cor. Bor.	62	41	0,07	62	41	0,6	+ 0,5
25	α Serpentis	84	0	36,52	84	0	36,6	+ 0,1
26	Antares	116	1	42,50	116	1	44,1	+ 1,6
27	α Herculis	75	23	59,70	75	24	0,0	+ 0,3
28	α Ophiuchi	77	18	9,74	77	18	10,6	+ 0,9
29	γ Draconis	38	29	10,31	38	29	10,5	+ 0,2
30	α Lyræ	51	22	30,37	51	22	30,2	+ 0,9
31	α Aquilæ	81	35	28,31	81	35	29,4	+ 1,1
32	α Cygni	45	20	50,80	45	20	52,4	+ 1,6
33	α Cephei	28	9	42,20	28	9	42,9	+ 0,7
34	β Cephei	20	12	53,78	20	12	54,1	+ 0,3
35	α Aquarii	91	10	28,85	91	10	31,3	+ 2,5
36	α Pegasi	75	44	38,52	75	44	41,7	+ 3,2
37	α Androm.	61	53	10,75	61	53	12,4	+ 1,7

TABLE III.

	Names of Stars.	Interpolated N. P. D. 1813. from 1756 & 1823.			Difference.	
					Dublin.	Greenwich.
		°	′	″	″	″
1	Polaris					
2	β Ursæ Min.	15	4	48,2	— 1,1	— 0,8
3	β Cephei	20	15	31,0	+ 0,1	+ 0,4
4	α Ursæ Maj.	27	14	31,0	— 0,7	— 0,6
5	α Cephei	28	12	13,6	+ 0,3	+ 1,0
6	α Cassiop.	34	29	24,1	+ 2,2	+ 1,3
7	γ Ursæ Maj.	35	15	54,8	— 0,7	— 0,5
8	γ Draconis	38	29	3,7	+ 0,7	+ 0,1
9	η Ursæ Maj.	39	44	58,1	+ 0,5	+ 0,2
10	α Persei	40	48	53,2	+ 2,6	+ 0,7
11	Capella	44	12	22,0	+ 2,1	+ 1,5
12	α Cygni	45	22	58,4	+ 0,9	+ 1,3
13	α Lyræ	51	23	1,2	+ 1,3	+ 0,6
14	Castor	57	42	47,9	+ 1,3	+ 1,1
15	Pollux	61	31	57,1	+ 2,0	+ 0,6
16	β Tauri	61	33	44,8	+ 1,6	+ 1,0
17	α Androm.	61	56	31,6	+ 2,3	+ 1,3
18	α Cor. Bor.	62	38	55,9	+ 1,5	+ 0,4
19	α Arietis	67	25	38,4	+ 2,7	+ 1,8
20	Arcturus	69	50	19,3	+ 1,1	+ 0,2
21	Aldebaran	73	52	36,7	+ 1,9	+ 1,4
22	β Leonis	74	22	57,5	+ 2,3	+ 0,2
23	α Herculis	75	23	14,3	+ 1,1	+ 0,3
24	α Pegasi	75	47	54,2	+ 2,5	+ 2,5
25	Regulus	77	7	23,0	+ 1,2	+ 0,3
26	α Ophiuchi	77	17	39,6	+ 0,4	+ 0,6
27	α Aquilæ	81	36	59,7	+ 1,2	+ 0,8
28	α Orionis	82	38	17,4	+ 2,8	+ 1,7
29	α Serpentis	82	58	39,4	+ 2,0	+ 0,1
30	Procyon	84	18	16,7	+ 2,8	+ 2,3
31	α Ceti	86	39	2,5	+ 1,9	+ 1,7
32	α Aquarii	91	13	24,0	+ 3,8	+ 2,4
33	α Hydræ	97	51	12,4	+ 3,0	+ 1,1
34	Rigel	98	25	35,5	+ 2,8	+ 1,7
35	Spica Virg.	100	10	51,3	+ 1,7	+ 0,5
36	Sirius.	106	28	4,2	+ 1,9	+ 2,7

TABLE IV.

	Names of Stars.	N. P. D. 1822. I.	No. of Obs.	N. P. D. 1822. II.	No. of Obs.		N. P. D. 1822. III.	No. of Obs.		N. P. D. 1822. IV.	No. of Obs.
					D.	R.		D.	R.		
		° ′ ″		° ′ ″			° ′ ″			° ′ ″	
1	Polaris	1 38 27,0	300							1 38 26,8	300
2	β Ursæ Min.	15 7 0,8	91	15 7 1,0	22	18	15 7 0,5	22	18	15 7 0,4	92
3	β Cephei	20 13 9,7	44	9,7	21	18	9,8	21	18	9,2	46
4	α Ursa Maj.	27 17 24,6	53	24,4	14	13	23,9	12	10	24,1	53
5	α Cephei	28 9 57,5	37	57,9	25	20	58,1	25	20	57,7	41
6	α Cassiopeiæ	34 26 25,6	51	26,0	12	8	25,9	12	8	25,3	56
7	γ Ursa Maj.	35 18 54,8	45							55,0	45
8	γ Draconis	38 29 9,8	120							9,7	120
9	η Ursæ Maj.	39 47 41,5	95							40,7	95
10	α Persei	40 46 52,5	48							53,1	51
31	Capella	44 11 41,4	75	41,4	25	20	41,5	25	20	41,6	75
12	α Cygni	45 21 5,0	80	5,1	40	40	5,4	40	40	5,1	84
13	α Lyræ	51 22 34,5	87	34,0	60	60	34,2	60	60	34,8	89
14	Castor	57 43 52,2	60	52,0	20	18	52,3	20	18	52,2	52
15	Pollux	61 33 9,1	60	8,8	15	15	8,8	15	15	8 9	50
16	β Tauri	61 33 10,5	50	10,3	16	16	10,5	16	16	10,2	46
17	α Androm.	61 53 32,6	30	32,6	12	10	32,4	12	10	32,4	37
18	α Cor. Bor.	62 40 48,3	26	48,1	16	16	48,1	22	18	48,3	32
19	α Arietis	67 23 1,6	44	1,8	24	17	2,0	24	17	1,6	49
20	Arcturus	69 53 10,2	84	10,2	21	22	10,5	21	22	10,0	84
21	Aldebaran	73 51 25,6	58	25,5	20	16	25,6	20	16	25,4	66
22	β Leonis	74 25 58,5	16	58,1	16	16	58,3	16	16	58,7	16
23	α Herculis	75 23 55,5	17	55,6	10	13	56,3	21	24	55,7	31
24	α Pegasi	75 45 1,0	30	1,1	24	20	1,4	24	20	1,4	34
25	Regulus	77 9 57,8	54	58,5	33	25	59,1	33	22	58,7	54
26	α Ophiuchi	77 18 7,6	20	7,8	10	12	7,6	21	25	7,3	34
27	α Aquilæ	81 35 38,7	70	38,6	44	42	38,9	44	42	39,0	86
28	α Orionis	82 38 5,7	48	5,6	12	12	6,1	14	14	5,7	54
29	α Serpentis	83 0 25,3	31	24,7	17	14	25,4	17	14	25,5	37
30	Procyon	84 19 34,8	57	34,6	10	7	34,9	10	7	34,5	63
31	α Ceti	86 36 51,4	46							51,2	46
32	α Aquarii	91 10 48,6	40	48,7	26	15	48,9	27	15	49,0	40
33	α Hydræ	97 53 29,2	18	29,4	16	12	29,4	16	12	30,0	18
34	Rigel	98 24 53,3	11							53,8	19
35	Spica Virg.	100 13 41,6	22	41,8	22	24	41,2	22	24	41,6	21
36	Sirius.	106 28 44,4	75	44,3	7	7	43,9	7	7	44,5	75
37	Antares.	116 1 35,5	5	35,4	5	5	35,6	5	5	36,0	5

No. I, is computed in the usual manner by direct measurement from the Pole.

No. II, by the angular distance of the stars from their reflected images in an artificial horizon, co-latitude assumed 38° 31′ 21″.

No. III, computed in the same manner as No. II, but from observations with two microscopes only.

No. IV, computed from observations with two microscopes in the same manner as No. I.

TABLE V.

		Error of Catalogue. I.	Error of Catalogue. II.	Error of Catalogue. III.	Error of Catalogue. IV.	
		"	"	"	"	
1	Polaris					
2	β Ursæ Min.	0,0	+ 0, 2	— 0, 3	— 0, 4	
3	β Cephei	0,0	0, 0	+ 0, 1	— 0, 5	
4	α Ursæ Maj.	+ 0,2	— 0, 1	— 0, 5	— 0, 3	
5	α Cephei	— 0,3	+ 0, 1	+ 0, 3	— 0, 1	
6	α Cassiop.					This star was found to be too near the zenith to be observed accurately by reflection.
7	γ Ursæ Maj.					
8	γ Draconis					
9	η Ursæ Maj.					
10	α Persei					
11	Capella	+ 0, 1	0, 0	+ 0, 1	+ 0, 3	
12	α Cygni	— 0, 1	0, 0	+ 0, 3	0, 0	
13	α Lyræ	+ 0, 3	— 0, 2	0, 0	+ 0, 6	Discordant from error of division.
14	Castor	0, 0	+ 0, 1	+ 0, 1	0, 0	
15	Pollux	+ 0, 1	— 0, 2	— 0, 2	— 0, 1	
16	β Tauri	+ 0, 1	— 0, 1	+ 0, 2	— 0, 2	
17	α Androm.	+ 0, 1	+ 0, 1	— 0, 1	— 0, 1	
18	α Cor. Bor.	+ 0, 1	— 0, 1	— 0, 1	+ 0, 1	
19	α Arietis	— 0, 1	+ 0, 1	+ 0, 3	— 0, 1	
20	Arcturus	0, 0	0, 0	+ 0, 3	— 0, 2	
21	Aldebaran	0, 0	— 0, 1	+ 0, 0	— 0, 2	
22	β Leonis	+ 0, 2	— 0, 2	+ 0, 0	+ 0, 4	
23	α Herculis	— 0, 1	0, 0	+ 0, 7	+ 0, 1	No. III. discordant from the observ^s. having been made at different seasons.
24	α Pegasi	0, 0	— 0, 1	+ 0, 2	+ 0, 2	
25	Regulus	— 0, 4	+ 0, 3	+ 0, 4	+ 0, 5	This star is reserved for future examination.
26	α Ophiuchi	+ 0, 2	0, 0	+ 0, 5	+ 0, 1	
27	α Aquilæ	0, 0	— 0, 1	+ 0, 2	+ 0, 3	
28	α Orionis	+ 0, 1	+ 0, 0	+ 0, 4	0, 0	
29	α Serpentis	+ 0, 3	— 0, 3	+ 0, 4	+ 0, 5	Discordant from the different seasons of observation, and requiring examination.
30	Procyon	+ 0, 2	0, 0	+ 0, 3	— 0, 1	
31	α Ceti					
32	α Aquarii	— 0, 1	0, 0	+ 0, 3	+ 0, 4	
33	α Hydræ	— 0, 1	— 0, 0	0, 0	+ 0, 6	
34	Rigel					
35	Spica Virg.	— 0, 2	0, 0	— 0, 6	— 0, 2	
36	Sirius.	+ 0, 1	0, 0	— 0, 4	+ 0, 2	
37	Antares.	0, 0	0, 0	+ 0, 1	+ 0, 5	
	Sum of Errors	3, 8	2, 3	8, 0	7, 3	
	Mean Error	0,13	0,08	0,26	0,25	

From the exact coincidence of Catalogue I. and II. it may be inferred that the assumed co-lat. 38° 31′ 21″,0 is extremely near the truth.

TABLE VI.

		N. P. D. 1822. Greenwich.			N. P. D. 1822. Dublin.	Difference in 1822.		Difference in 1812.		Difference between Bessel and Greenwich.	
		°	′	″	″	″		″		″	
1	Polaris	1	38	26,9	26,7	— 0,2					
2	β Ursæ Min.	15	7	0,8	1,8	+ 1,0					
3	β Cephei	20	13	9,8	9,8	0,0					
4	α Ursa Maj.	27	17	24,3	24,1	— 0,2		— 1,2			
5	α Cephei	28	9	58,0	57,2	— 0,8		+ 0,8			
6	α Cassiop.	34	26	25,8	23,3	— 2,5	— 0″,6	— 0,8	— 0″,3		
7	γ Ursæ Maj.	35	18	54,4	54,5	— 0,1		+ 0,3			
8	γ Draconis	38	29	9,8	8,9	+ 1,0		— 0,6			
9	η Ursæ Maj.	39	47	41,6	41,0	— 0,6		— 0,3			
10	α Persei	40	46	52,5		— 1,1					
11	Capella	44	11	41,4	39,8	— 1,6		- 0,6		+ 0″,5	
12	α Cygni	45	21	5,0	3,7	— 1,3		+ 0,4		+ 1,8	
13	α Lyræ	51	22	34,2	32,9	— 1,3		— 0,5		+ 2,2	
14	Castor	57	43	52,0	50,7	— 1,3		— 0,1		+ 1,3	
15	Pollux	61	33	9,0	8,2	— 0,8		— 1,2		+ 1,5	
16	β Tauri	61	33	10,4	9,4	— 1,0		— 0,5		+ 1,7	
17	α Androm.	61	53	32,5	30,9	— 1,6	— 1,3	0,3	— 0,7	+ 1,0	+ 1″,5
18	α Cor. Bor.	62	40	48,2	46,8	— 1,4		— 1,0		+ 2,3	
19	α Arietis	67	23	1 7	0,2	— 1,5		— 0,7		+ 1,1	
20	Arcturus	69	53	10,2	9,5	— 0,7		— 0,9		+ 2,3	
21	Aldebaran	73	51	25,6	24,2	— 1,4		— 0,7		+ 1,4	
22	β Leonis	74	25	58,1	56,6	— 1,5		— 2,0		+ 2,0	
23	α Herculis	75	23	55,5						+ 3,3	
24	α Pegasi	75	45	1,0						+ 1,3	
25	Regulus	77	9	58,2	58,1	— 0,1		— 0,9		+ 2,8	
26	α Ophiuchi	77	18	7,4	6,2	— 1,4		0,0		+ 3,2	
27	α Aquilæ	81	35	38,5	36,6	— 1,9		— 0,4		+ 2,7	
28	α Orionis	82	38	5,6	4,0	— 1,6		— 1,1		+ 1,0	
29	α Serpentis	83	0	24,9	23,5	— 1,4	— 1,9	— 1,9	— 0,8	+ 3,7	+ 2,4
30	Procyon	84	19	34,6	32,9	— 1,7		— 0,5		+ 2,3	
31	α Ceti	86	36	51,4	49,5	— 1,9		— 0,1		+ 1,7	
32	α Aquarii	91	10	48,6	45,7	— 2,9		— 1,7		+ 2,3	
33	α Hydræ	97	53	29,3						+ 2,8	
34	Rigel	98	24	53,2						+ 1,5	
35	Spica Virg.	100	13	41,8	40,9	— 0,9				+ 3,8	+ 2,4
36	Sirius.	106	28	44,3	42,3	— 2,0	— 2,0			+ 1,7	

6. α Cassiop. I suspect some mistake in the computations of this star; I have therefore in taking the mean, substituted α Persei for it, which Dr. Brinkley was so obliging as to send me a few days since.

25. Regulus. There is probably also some mistake relative to this star.

36. Sirius. By a number of observations made last year at the same period, and computed by the same equations: the two results differ exactly 2″. This seems therefore to be the quantity by which the two instruments differ in measuring an angle of 100°.

TABLE VII.

		Bradley's refract. N. P. D. 1820. Dr. Brinkley.	N.P. D 1820. Mr. Bessel.	Difference between Dr. Brinkley and Mr. Bessel.
		° ′ ″		
1	Polaris	1 39 5, 6		
2	β Ursæ Min.	15 6 32, 2		
3	β Cephei	20 13 41, 0		
4	α Ursæ Maj.	27 16 45, 8		
5	α Cephei	28 10 27, 3		
6	α Cassiopeiæ	34 27 3, 0		
7	γ Ursæ Maj.	35 18 14, 5		
8	γ Draconis	38 29 7, 5		
9	η Ursæ maj.	39 47 4, 8		
10	α Persei		° ′ ″	″
11	Capella	44 11 48, 8	50,88	2,1
12	α Cygni	45 21 28, 9	31,53	2,6
13	α Lyræ	51 22 39, 0	42,23	3,2
14	Castor	57 43 36, 3	38,95	2,6
15	Pollux	61 32 52, 0	54,46	2,5
16	β Tauri	61 33 16, 9	19,60	2,7
17	α Androm.	61 54 10, 7	13,41	2,7
18	α Cor Bor.	62 40 21, 9	25,51	3,7
19	α Arietis	67 23 34, 7	37,68	3,0
20	Arcturus	69 52 31, 5	34,57	3,0
21	Aldebaran	73 51 40, 1	42,84	2,7
22	β Leonis	74 25 16, 5	19,96	3,5
23	α Herculis			
24	α Pegasi			
25	Regulus	77 9 23, 5	26,42	2,9
26	α Ophiuchi	77 18 0, 0	4,34	4,3
27	α Aquilæ	81 35 54, 7	59,31	4,6
28	α Orionis	82 36 6, 5	9,34	2,8
29	α Serpentis	83 0 0, 0	5,16	5,2
30	Procyon	84 19 15, 5	19,68	4,2
31	α Ceti	86 37 18, 3	22,33	5,0
32	α Aquarii	91 11 20, 5	25,48	5,0
33	α Hydræ		97 53 1,68	
34	Rigel			
35	Spica Virg.	100 13 4, 4	7,69	3,3
36	Sirius	106 28 33, 1	37,15	4,0
37	Antares.			
	γ Aquilæ.	79 49 1, 6	6,03	4,4
	β	84 2 3, 3	9,16	5,9
	1 } α Capri.	103 3 19,60	25,59	6,0
	2 } α Capri.	103 5 37,03	43,49	6,5
	γ Pegasi.	75 48 59, 3	49 3,78	4,5

TABLE VIII.

General Catalogue of Stars for the year 1813.

1756 & 1813. An. Var. 1818.	Number.	Names of Stars.	Æ. 1823,	Predicted N. P. D. 1823.	Observed N. P. D. 1823. Bradley's Refraction.	Stars observed South.	No. of Observations.			Interpolated Catalogue N. P. D. 1818. $\frac{1813 + 1823}{2}$.	No. of Observations.
							1756.	1812. 1813.	1822. 1823.		
			h. m. s.	° ′ ″	° ′ ″					° ′ ″	
— 20,09	1	γ Pegasi	0 4 8,09	75 48 0,11	75 48 2,4	2,3	14	25	13	76 49 41,7	38
— 19,85	2	α Cassiopeiæ	0 30 31,29	34 26 4,23	34 26 5,7	1,5	5	77	19	34 27 44,3	136
	3	Polaris*	0 57 46,4		1 38 7,5			200	300	1 39 44,5	500
— 17,40	4	α Arietis	1 57 13,09	67 22 42,60	67 22 44,4	1,8	10	77	91	67 24 10,6	168
— 14,59	5	α Ceti	2 53 2,29	46 36 34,86	86 36 36,8	2,0	7	18	31	86 37 48,7	49
— 13,41	6	α Persei	3 11 44,48	40 46 38,34	40 46 39,0	0,7	10	66	48	40 47 45,7	114
— 7,92	7	Aldebaran	4 25 46,58	73 51 16,17	73 51 17,7	1,5	60	69	74	73 51 56,4	143
— 4,54	8	Capella	5 3 37,83	44 11 35,11	44 11 36,9	1,8	59	108	95	44 11 58,8	203
— 4,74	9	Rigel	5 6 2,21	98 24 46,44	98 24 48,4	2,0	37	30	11	98 25 11,2	41
— 3,80	10	β Tauri	5 15 6,76	61 33 5,74	61 33 6,7	1,0	14	70	66	61 33 25,0	136
— 1,36	11	α Orionis	5 45 35,66	82 38 2,11	82 38 4,2	2,1	69	66	62	82 38 9 9	128
+ 4,41	12	Sirius	6 37 20,85	106 28 45,35	106 28 48,7	3,4	84	30	82	106 28 25,1	112
+ 7,12	13	Castor	7 23 17,62	57 43 58,08	57 43 59,1	1,0	19	47	78	57 43 23,0	125
+ 8,63	14	Procyon	7 30 2,19	84 19 40,75	84 19 43,2	2,4	64	40	64	84 18 58,9	104
+ 8,02	15	Pollux	7 34 28,52	61 33 16,76	61 33 17,1	0,3	38	53	75	61 32 36,8	128
+ 15,19	16	α Hydræ	9 18 53,50	97 53 43,20	97 53 44,5	1,3	5	10	30	97 52 27,9	40
+ 17,23	17	Regulus	9 58 56,36	77 10 15,00	77 10 15,4	0,4	25	68	79	77 8 49,2	147
+ 19,26	18	α Ursæ Maj.	10 52 43,41	27 17 44,16	27 17 43,6	— 0,6	7	76	80	27 16 7,6	150
+ 20,04	19	β Leonis	11 40 1,66	74 26 17,73	74 26 18,1	0,4	11	22	32	74 24 37,7	54
+ 19,98	20	γ Ursæ Maj.	11 44 28,63	35 19 15,18	35 19 14,8	— 0,4	5	60	45	35 17 34,9	105
+ 18,94	21	Spica Virg.	13 15 52,91	100 14 0,73	100 14 0,7	0,0	19	20	46	100 12 26,0	66
+ 18,15	22	η Ursæ Maj.	13 40 33,54	39 47 59,60	39 47 59 5	— 0,1	Z. Sect.	100	95	39 46 28,9	195
+ 18,97	23	Arcturus	14 7 35,61	69 53 28,83	69 53 29,2	0,4	106	120	106	69 51 54,1	226
+ 15,30	24	1 } α Libræ	14 40 54,93	105 15 11,91	105 15 12,5	0,6	4	12	14	105 13 56,8	26
+ 15,32	25	2 }	14 41 6,36	105 17 55,67	105 17 56,3		11	15	20	103 16 39,5	35
+ 14,74	26	β Ursæ Min.	14 51 19,53	15 7 16,38	15 7 15,6	— 0,8	1	110	109	15 6 2,3	219
+ 12,45	27	α Cor. Bor.	15 27 11,95	62 41 0,07	62 41 0,6	0,5	7	90	42	62 39 58,1	132
+ 11,72	28	α Serpentis	15 35 33,53	83 0 36,52	83 0 36,6	0,1	5	77	45	82 59 38,0	122
+ 8,59	29	Antares	16 18 34,23	106 1 42,50	116 1 44,1	1,6	21	36	10	116 1 0,1	46
+ 4,57	30	α Herculis	17 6 35,00	75 23 59,70	75 24 0,1	0,4	7	55	30	75 23 37,0	85
+ 3,08	31	α Ophiuchi	17 26 43,49	77 18 9,74	77 18 10,6	0,9	3	83	32	77 17 54,7	115
+ 0,67	32	γ Draconis	17 52 30,14	38 29 10,31	38 29 10,5	0,2	Z. Sect.	140	120	38 29 7,1	260
— 3,02	33	α Lyræ	18 30 56,98	51 22 30,37	51 22 31,2	0,8	89	170	147	51 22 45,9	317
— 8,34	34	γ }	19 37 50,82	79 48 35,58							
— 9,06	35	α } Aquilæ	19 42 8,94	81 35 28,31	81 35 29,5	1,2	104	140	112	81 36 14,2	252
— 8,56	36	β }	19 46 37,27	84 1 37,94							
— 10,66	37	1 } α Capri.	20 7 49,91	103 2 48,83	103 2 49,6	1,0	6	35	22	103 3 42,5	57
— 10,68	38	2 }	20 8 13,69	103 5 5,45	103 5 6,6		10	28	22	103 5 59,4	50
— 12,63	39	α Cygni	20 35 24,19	45 20 50,80	45 20 52,4	1,6	55	120	120	45 21 54,5	240
— 15,07	40	α } Cephei	21 14 21,05	28 9 42,20	28 9 42,8	0,6	5	70	50	28 10 57,7	127
— 15 68	41	β }	21 26 20,44	20 12 53,78	20 12 54,0	0,2	5	70	62	20 14 12,5	132
— 17,27	42	α Aquarii	21 56 41,55	91 10 28,85	91 10 31,4	2,5	9	20	55	91 11 56,4	75
	43	Fomalhaut	22 47 50,97								
— 19,32	44	α Pegasi	22 55 57,20	75 44 38,52	75 44 41,8	3,3	6	30	50	75 46 16,7	80
— 19,95	45	α Andromedæ.	23 59 15,61	61 53 10,75	61 53 12,5	1,8	11	40	40	61 54 51,4	80

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* The mean of about 1300 observations of the pole star during the last ten years, is 1° 39′ 44″,5 for the N.P.D. for Jan. 1, 1818, and the mean of all the annual variations 19″,42 or 19″,43.

TABLE IX.

Observations of α Lyræ.

Two Microscopes.

Summer.

Direct. 1822.	46° 22′ ″	Reflection. 1822.	200° 40′ ″
July 3	32,82	July 1	7,94
4	34,64	6	8,64
10	34,88	14	8,51
17	34,92	19	6,59
20	34,38	31	8,25
21	35,28	Aug. 2	7,73
24	34,94	5	8,63
25	34,09	11	8,05
28	35,15	12	8,55
30	34,39	14	9,11
Aug. 4	33,36	16	8,29
8	34,80	18	9,96
10	35,82	20	9,81
13	34,79	21	8,07
15	34,40	23	8,26
17	33,70	25	8,04
19	35,68	26	7,18
22	35,88		
27	35,07		

Mean of 19 = 34,63 Mean of 17 = 8,45

$\frac{R-D}{2}$ = 16,91″ of altitude.

Autumn.

Direct. 1822.	46° 22′ ″	N. P. D. 1822.	Reflection. 200° 40′ ″
Aug. 29	34,41		
Sept. 3	34,53	Aug. 31	7,37
4	34,32	Sep. 1	7,19
6	35,13	7	7,59
8	34,35	10	7,12
11	32,75	13	7,55
14	33,87	19	9,02
16	34,39	20	7,44
18	34,76	21	10,24
28	35,15	29	8,73
30	35,37	Oct. 3	9,00
Oct. 5	36,59	4	9,52
6	34,79	8	8,30
7	34,86	11	7,03
12	34,49	18	8,84
21	34,23	22	7,70
23	36,57	25	8,43
26	34,50	27	9,00
28	35,31		

Mean of 19 = 34,76 Mean of 17 = 8,24

$\frac{R-D}{2}$ = 16,74″ of altitude.

Winter.

Direct. 1822.	46° 22′ ″	Reflection. 1822.	200° 40′ ″
Nov. 3	36,05	Oct. 31	9,15
4	35,64	Nov. 7	8,58
11	34,63	21	9,74
13	35,68	27	8,70
15	34,52	Dec. 2	7,89
18	36,07	3	8,43
20	36,28	7	8,88
23	35,81	26	9,02
Dec. 5	33,79	28	10,26
8	34,77	29	9,07
10	35,09	1823.	
11	35,82	Jan. 8	7,63
21	35,50	11	8,83
22	33,90	13	6,94
26	33,84	Feb. 7	7,22
1823.		8	8,84
Jan. 18	35,83	12	8,57
Feb. 3	35,15	18	6,75
4	35,06	19	8,71
13	34,46	25	8,85
23	35,28	Mar. 1	7,95

Mean of 20 = 35,16 Mean of 20 = 8,49

$\frac{R-D}{2}$ = 16,67″ of altitude.

TABLE X.

Observations of α Lyræ.

Six Microscopes.

Summer.

1822.	Direct. 46° 22′ ″	1822.	Reflection. 200° 40′ ″
July 3	33,42	July 1	8,54
4	34,84	6	8,94
10	34,28	14	8,31
17	34,32	19	7,29
20	34,58	31	7,35
21	34,68	Aug. 2	8,13
24	33,94	5	8,53
25	33,99	11	8,85
28	34,45	12	8,85
30	33,39	14	9,61
Aug. 4	33,26	16	8,39
8	34,45	18	8,86
10	35,82	20	9,81
13	34,79	21	7,76
15	34,40	23	8,06
17	33,48	25	7,64
19	35,88	26	8,08
22	34,98		
27	34,47		

Mean of 19 = 34,39 Mean of 17 = 8,41

$\frac{R-D}{2} = 17{,}01''$ of altitude.

Autumn.

1822.	Direct. 46° 22′ ″	1822.	Reflection. 200° 40′ ″
Aug. 29	34,71		
Sept. 3	35,13	Aug. 31	8,27
4	35,12	Sept. 1	7,99
6	35,36	7	8,29
8	34,85	10	7,37
11	32,35	13	7,95
14	34,17	19	8,52
16	33,69	20	7,14
18	34,86	21	9,34
28	33,85	29	8,33
30	34,47	Oct. 3	8,79
Oct. 5	35,08	4	9,61
6	35,68	8	8,41
7	34,75	11	7,42
12	35,09	18	8,63
21	35,12	22	8,29
23	34,30	25	8,51
26	33,68	27	8,08
28	33,89		

Mean of 19 = 34,53 Mean of 17 = 8,29

$\frac{R-D}{2} — 16{,}88''$ of altitude.

Winter.

1822.	Direct. 46° 22′ ″	1822.	Reflection. 200° 40′ ″
Nov. 3	34,63	Oct. 31	8,13
4	34,42	Nov. 7	8,26
11	34,21	21	9,54
13	34,46	27	9,60
15	33,30	Dec. 2	8,69
18	34,35	3	8,73
20	34,76	7	8,68
23	34,49	26	8,67
Dec. 5	33,69	28	10,31
8	34,57	29	9,22
10	34,69	1823.	
11	35,72	Jan. 8	8,55
21	35,35	11	8,85
22	33,65	13	7,96
26	35,31	Feb. 7	7,27
1823.		8	7,70
Jan. 18	34,30	12	8,12
Feb. 3	34,32	18	7,60
4	34,64	19	8,76
13	34,35	25	8,00
23	34,55	Mar. 1	8,09

Mean of 20 = 34,49 Mean of 20 = 8,54

$\frac{R-D}{2} = 17{,}02''$ of altitude.

TABLE XI.

α Lyræ compared with γ Draconis.

		No. of Obs. γ Draconis.	No. of Obs. α Lyræ.		°	′	″
1812	Summer	30	24		12	53	56,77
1813	Summer	36	40		12	53	56,77
		Mean of 2 years			12	53	56,77
1812	Winter	16	23		12	53	57,02
1813	Winter	15	30		12	53	56,80
		Mean of 2 years			12	53	56,91
		Double Parallax of α Lyræ					0,14
1822	Winter	24	30	+	12	53	24,7
1822	Summer	50	25		12	53	24,7
1823	Winter	40	27		12	53	24,7
		Double Parallax of α Lyræ					0,0

Explanation of the preceding Tables.

Table I. The predicted catalogue in this table is obtained from the Greenwich Catalogues of 1756 and 1813: all the computations will be found at length in the volume of the Greenwich Observations for 1820. The five groupes of stars in the last column are those referred to in page 42.

Table II. is the same catalogue arranged in the order of right ascension.

Table III. is an interpolated catalogue: it was computed some time since from a catalogue less perfect than the present, but it is sufficiently exact to show that no explanation of the difficulty can be obtained by supposing any defect in the observations of 1813. The numbers in this table under the columns Dublin and

Greenwich, are the quantities that must be applied to the Dublin and Greenwich observations to produce the interpolated catalogue.

Table IV. contains four catalogues, in which, as no systematic difference can be traced, the instrument must be considered as perfect within the limits of the small discordances in Catalogue I. and II.

Table V. contains the errors of each of the preceding catalogues. From this table it appears, that the regular difference between the results with six and with two microscopes, is now nearly insensible. This must have arisen formerly from flexure; and the new braces, though intended only to strengthen the attachment of the telescope to the circle, have, in fact, added strength and firmness to the whole frame of the instrument. (Vide Experiments on this subject in the volume of Observations for 1820.)

Table VI. shows the difference between the results of the Dublin and Greenwich circle, both at the present time, and in the year 1812. From this it is evident that a small change has taken place in one of the instruments. Formerly the two instruments differed only one second in an arc of 90°; at present, the difference amounts very exactly to double that quantity.

Table VII. contains the two catalogues of Dr. Brinkley and Mr. Bessel. Here the differences are much greater and more irregular.

Table VIII. contains a general catalogue of the stars, including several that were not very accurately observed in 1813; but which, nevertheless, confirm in a remarkable manner the general law of southern deviation.

Tables IX, X and XI. contain observations of α Lyræ, by which it appears, that whatever may be the parallax of this star, it is not within the powers of our instrument to detect it. With Dr. Brinkley's Refraction, the result would have been a very small fraction of a second less in favour of parallax.

VIII. *Observations on the heights of places in the Trigonometrical Survey of Great Britain, and upon the Latitude of Arbury Hill.*

By B. BEVAN, *Esq.* *Communicated by Sir* H. DAVY, *Bart. P. R. S.*

Read May 23, 1822.

THE Trigonometrical Survey of Great Britain having from time to time engaged the attention of the Royal Society, and circumstantial particulars of this great national undertaking having occupied the pages of the Philosophical Transactions, I beg leave to submit a few observations on that subject to the consideration of the Society.

The result of the survey, relative to the different sections of the meridian, in this country, has not altogether proved so satisfactory as might have been expected.

I have lately examined the calculations affected by the observations made at *Arbury Hill* in the county of Northampton, with some hope of discovering the means of reconciling the anomaly in that part of the meridian.

I have been at the expense of having the height of this station determined by accurate levelling to the Grand Junction Canal, from which, and the known difference of level of the various canals connected with this, I have been able to find the *relative* height of this station, with most of the important objects in the counties of Northampton, Buckingham, and Bedford.

From this operation of levelling, I found the country to the north of Arbury station, suddenly to fall about 400 feet, and continue at this depressed state for 9 or 10 miles. Such a defect of matter, to the north of the station, was in itself a

strong ground for supposing a deflection of the plumb-line to the southward. To ascertain if this supposition were supported by the trigonometrical operations, I calculated the latitude of Arbury station, from the latitude of Blenheim, as determined by previous observation, independent of any astronomical observation made at Arbury, and find it 5 *seconds* LESS than shown by the zenith sector; giving countenance to the probability of local attraction by the high land to the *south* of the station, which will appear by the following calculation; in Trigonometrical Survey, Vol. 2, p. 137, the latitude of Blenheim by observation is stated as 51° 50′ 24″.9 or nearly 51° 50′ 25″.

The measured distance, on the meridian, from Blenheim to Arbury, is 139822, deduced from Vol. 2, part 2, p. 107; dividing this distance by 60881.7, according to a table in Vol. 1, part 1, page 168, corresponding to the middle latitude between Blenheim and Arbury, and multiplying the quotient by 10. we obtain the difference of latitude = ° 22′ 58″.
this quantity added to - - - - 51 50 25
gives for the latitude of Arbury - - - 52 13 23
being 5″ *less* than by the zenith sector. It is true that Colonel MUDGE corrected the latitude of Blenheim, from the Trigonometrical Survey, to 51° 50′ 28″, and in this case the computed latitude of Arbury would come out near 3 seconds *less* than found by the zenith sector. In Vol. 2, part 1, page 118, the *latitude of Arbury*, as derived from Dunnose meridian, is
given - 52° 13′ 26″.6
afterwards 52 13 28 .2 from observation*
1 .6 or $1\frac{1}{2}$ second *south* of the observed

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* Vol. 2, part 2, page 109, Dunnose is stated to be . . 50° 37′ 8″,2
p. 107, from Dunnose to Arbury is . 1 36 19,98
52 13 28,18

latitude; all concurring to prove that the observed latitude by zenith sector falls to the north of the calculated, or that the deflection of the plumb-line was to the SOUTH: taking, therefore, the table above referred to in Vol. 1, part 1, p. 168, and considering the latitudes of the following stations to be as below:

Dunnose - - - -	51°	37′	7″
Greenwich - - -	51	28	39½
Blenheim - - -	51	50	28½
Arbury - - - -	52	13	26½
Clifton - - - -	53	27	20¾

and calculating the length of a degree, in their respective middle points, they will be found to correspond with the said table, and maintain a *regular increase to the northward*, agreeing with the assumed general figure of the earth: the above assumption indicates an error of 10¾″ at Clifton, and 1¾ at Arbury, neither of which is more than might be expected from the visible inequality of the contiguous land.

The result of the operations north of Clifton I have not had an opportunity of ascertaining; but it appears to me that a few more observations by the zenith sector, at other stations, would remove much of the apparent ambiguity at present attached to this interesting question.

Knowing the goodness of the instrument used in the Survey, and the great skill and attention observed by the persons engaged, I have great confidence in the general result of the terrestrial department. I should have been doubly gratified if I could have said as much on the determination of the *heights* of the stations.

Availing myself of the levels through a long district of the

Grand Junction Canal, I have been at the trouble of levelling from the following stations, viz.

Wendover Down,
Kensworth,
Bowbrick Hill, and
Arbury Hill,

to the nearest point of the said Canal, and thus, by means of the known level of the different parts of the Canal, to obtain the *relative heights* of the above mentioned stations.

And as a comparison will be more readily made from a table of heights expressed in positive numbers, I shall assume the highest point of the summit of said Canal to be 402 feet above the level of the sea, at low water spring tides; with this assumption, the heights of the several stations, in feet, above low water mark, will be as follows:

Wendover Station - - - -	861
Kensworth ditto - - - -	809½
Bowbrick Hill - - - - - -	571½
Arbury Hill - - - - - - -	740½

The heights of these Stations published in the Philosophical Transactions, are as below: in Vol. 3, page 302.

Wendover - - - - - - -	905
Kensworth - - - - - -	904
Bowbrick Hill - - - -	683
Arbury Hill - - - - - -	804

these will average about 78 feet higher than in the table above.

I have also levelled from the summit of the Regent's Canal, to the mouth of the fixed cannon at King's Arbour, or the upper end of the base on Hounslow Heath, and upon the same data this point will be 90¾ feet above low water mark.

In Vol. 1, p. 173, Colonel Mudge gives 91¼ for its height, which differs only by half a foot.

But at page 266 it is stated to be 118 *feet*, being 27 feet above the proper height. Again in Vol. 3, p. 307, it is stated to be 132 *feet*, or 41¼ too high.

The grounds of my assumption of 402 feet being the height of the Grand Junction Canal summit, near Tring, are these:

The range of tide, from low water spring tides at sea, to high water near Somerset House, I presume to estimate 19⅓ feet; from this point to the Regent's Canal summit will be found 83⅙ feet; from this level I apply the revised section of the Grand Junction Canal = 299½ to the summit near Tring, making together 402 feet as above.

From these levels it will appear, that Wendover Station above Brickhill is 861 — 571½ = 289½. Colonel Mudge's numbers give - - 905 — 683 = 222

67½ error.

Arbury above Brickhill 740½ — 571½ = 69
Colonel Mudge - - 804 — 683 = 121
Error 52

Wendover above Arbury 861 — 740½ = 120½
Colonel Mudge - - 905 — 804 = 101
Error 19½

Some fresh observations with the zenith sector made at Blenheim, and Sutton, would offer a fine check to the latitude of Arbury; and also at Highbeach, and Botley Hill, a check to the latitude of Greenwich would be readily obtained.

B. Bevan.

Leighton, Bedfordshire,
February, 1822.

IX. *On some Fossil Bones discovered in Caverns in the Limestone Quarries of Oreston. By* JOSEPH WHIDBEY, *Esq. F.R.S. In a Letter addressed to* JOHN BARROW, *Esq. F. R. S. To which is added, a Description of the Bones by Mr.* WILLIAM CLIFT, *Conservator of the Museum of the College of Surgeons.*

Read February 6, 1823.

DEAR SIR, Plymouth, 19th August, 1822.

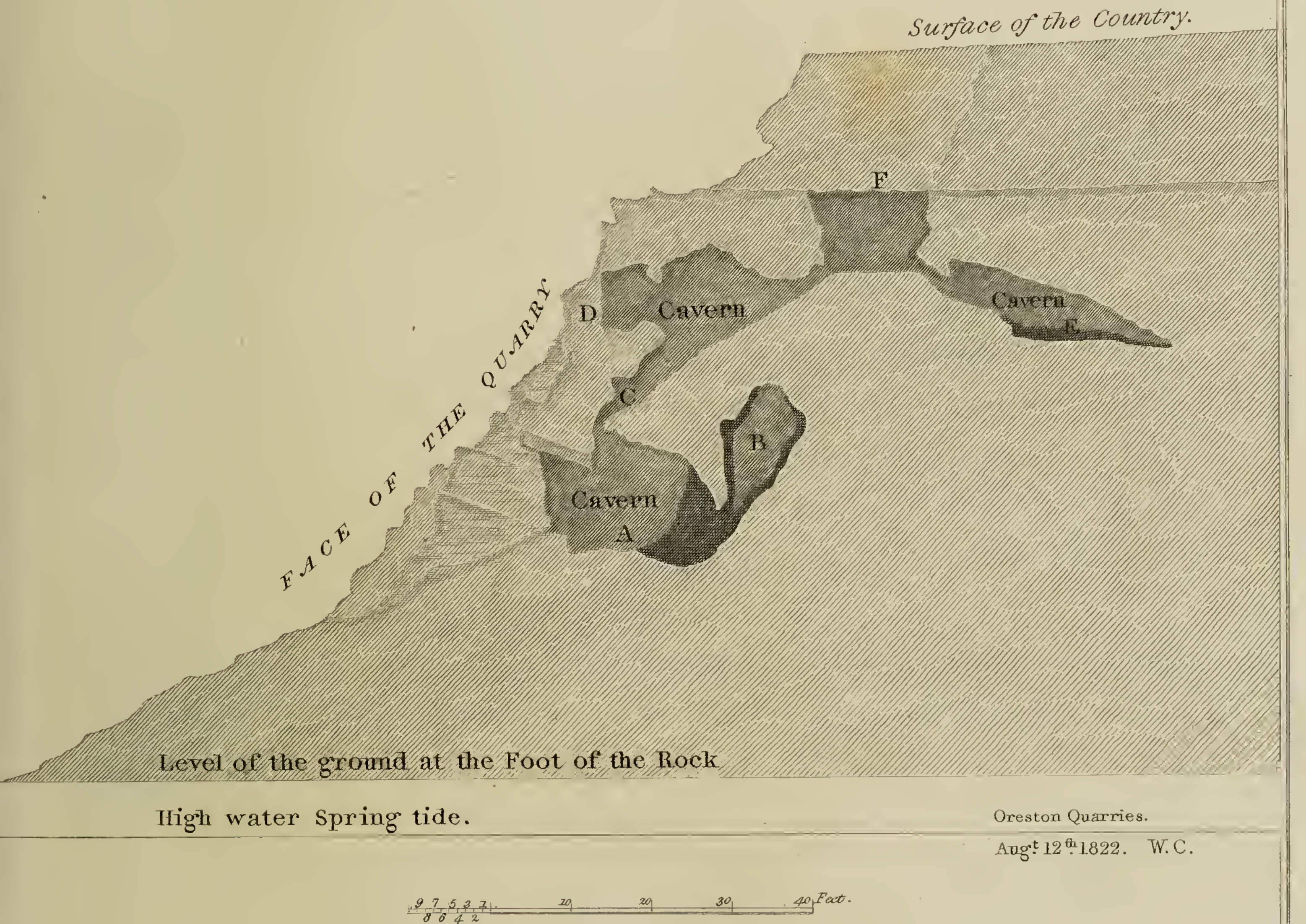
IN continuing to quarry the lime-stone rock at Oreston, in Catwater, near Plymouth, for the use of the Breakwater, the workmen came to another cave, containing many teeth and bones, which I have sent to you to be disposed of as you may please to direct for the benefit of science, this being the third cave, found in these quarries, containing teeth and bones: an account of the two former have been already published in the Philosophical Transactions.

It is not my intention to give any description of the fossils now found, but I shall content myself with merely describing the situation in which they were discovered, together with the state and appearance of the rock.

The height of the rock, or quarry, is about 93 feet above the top of high water of spring tides, which is shown in the sketch No. 1, [Plate VI.] together with a section of the caves where the bones were found. The part where they lay is tinged with red, in the caves marked A and B. The cave A, is encrusted with thin stalactite, some pieces of which

Sketch No. 1.

A LONGITUDINAL SECTION of the CAVERNS lately discovered in the BREAKWATER QUARRIES at ORESTON.



Oreston Quarries.
Aug.t 12th 1822. W. C.

REFERENCE

The distance from the entrance of the Caverns to the Original high water mark is 201 Yards. The dark Tint on the Section describes the places where the Bones were found.

F. Basire sculp.t

are in the case No. 2. The cave B mostly consists of lime-stone, with bones adhering to its sides: the top is closed up with stone rubble. The teeth and bones found in the cave A, were mostly covered with dirt, some of which will be found in all the cases; part of the bones were lying on the dirt, and in crevices about the caves A and B.

From the cave marked A, a passage has been discovered into what I call a gallery, marked C, which gallery opens into the face of the quarry at D. At E, some teeth and bones were found, which are contained in the case No. 5. The farther end of the gallery is not closed, but it is not sufficiently wide for a man to creep into it. The sides of the gallery consist mostly of lime-stone, some clay, and stalactite. At F, the gallery was covered with masses or lumps of lime-stone, with much clay intermixed, and in general so compact that it required gunpowder to blast it asunder; and continued so to the surface of the country, a height of 15 feet, as shown in the sketch.

The general state of this quarry has been found to consist of more caves filled with clay, than any other; and nearly under the entrance of the cave where the bones were found, I have dug down through clay of so stiff and hard a nature as to render it difficult to dig into it, and it continued so until I got to six feet below high water, when rock again appeared, but not compact. In this digging many lumps of iron ore were found in the hard clay.

I have introduced a scale into the sketch, from which any distance may be measured.

The distance from the cave A, to the commencement of the quarry or harbour, is 201 yards, and to the cave where

the first bones were found in November, 1816, 180 yards in a western direction.

The sketch No. 2, [Plate VII.] shows the face of the rock where the teeth and shells were found, which are in the case No. 8, with some of the clay and sand in which they lay. The face of the rock is very compact lime-stone, excepting the part shaded dark. In the lower part of it is a cave marked G, H, I, filled with the substance that will be found in case No. 8. In this cave the teeth were found at G, and the shells at H. Between I and K, the rock appears to be compact, but from K to L it is composed of narrow fissures, covered with thin stalactite throughout. At M, there is a small opening of but little extent; above and below which, and the fissures marked K and L, as well as the cave marked G, H, I, there is no apparent opening to lead to the surface, more than what I have mentioned in my description of sketch No. 1; but what may be produced in quarrying farther on I cannot tell, though I shall not fail to take notice as the quarry proceeds.

The height of this quarry to the surface at N, is 110 feet; and this part of the quarry is 400 feet distant from the cave A, in sketch No. 1, and is in fact part of the same quarry.

Professor Buckland and Mr. Warburton have examined the Oreston quarries and caves that contained the bones, and they also found some themselves.

This body of lime-stone rock commences at Mount Edgecombe, crosses the east end of the Dock-yard, and takes the line of road towards London, and I believe ends at Chudleigh, as I have never seen it to the eastward of that place. Both sides of Torbay are of lime-stone, but it continues no farther to the eastward in that direction. In what manner

Sketch No. 2.

Js. Basire sc.

the lime-stone at Torbay is connected with Plymouth and Chudleigh, or whether it be connected or not, I am unable to say,

I am, Dear Sir, &c. &c.
JOSEPH WHIDBEY.

N. B. Since the bones were packed up, another jaw bone has been found, more perfect than any I have seen, with most of the teeth in it. This was found in cave B, where many of the other bones were discovered.

A description of the Fossil Bones found in the Caverns of Oreston, referred to in the foregoing Paper. By Mr. William Clift, *Conservator of the Museum of the Royal College of Surgeons, in which the bones are deposited.*

On receiving this large collection of fossil bones for the purpose of comparing them with those of recent animals, and with other specimens, now deposited in the Museum of the College, which were discovered in caverns in the same lime-stone rock in the years 1816 and 1820, I immediately perceived that they belonged chiefly, if not entirely, to animals of different genera from those formerly met with; and consequently, I became desirous of ascertaining how far they were similarly circumstanced in regard to the relative situations in which they were found; because in the caverns first discovered, the bones of the different species were entirely separate from each other.

In the cavern discovered in 1816, although the greatest care was taken to collect every bone contained in it, those of the rhinoceros alone were found.

In those of 1820, one cavern contained bones and teeth of the bear; while another contiguous cavity, of apparently coeval formation, contained only bones of a deer or antelope; but of which, the genus could not be positively ascertained, as neither the teeth, nor horns, nor any part of the head were found.

In the caverns discovered in 1822, which form the subject of the present communication, the bones of animals of several distinct genera were found; namely, the bos, the deer, the horse, the hyæna, the wolf, and the fox. These cavities, however, communicated with each other, and the bones of the different graminivorous animals were found mingled together in the same cavity; but those of the carnivora at a considerable distance from each other; the bones of the hyæna having been discovered in the cavern marked B in the annexed plan, and those of the wolf and fox in the gallery marked E.

Some of the bones and fragments of bones which lay on and near the surface of the clay, have acquired a thin crust of stalagmite, but in none of them does it appear to have penetrated beyond the surface: the greater number were imbedded in the stiff clay, which adhered so firmly to them, that many were broken by the workmen in separating them from the matrix; and others have fallen to pieces since their exposure to the air; but a great proportion of the cylindrical and other bones, of the graminivorous animals in particular, are still as perfect in form as at the time of the death of the animals to which they belonged, and do not exhibit the least appearance of having been gnawed or otherwise mutilated.

The only specimen in this very large assemblage which

bears any apparent marks of teeth, is a portion of the radius of a young wolf, which, in two or three places on its surface, has the impression of the incisors and canine teeth of some small animal of the size of a weasel.

The clay still adhered so firmly to the surface of many of these bones, that unless removed with considerable caution, the outer layers separated along with it, and showed that but little animal matter remained; and on submitting some of these fragile portions to the action of dilute muriatic acid, they almost intirely dissolved, leaving scarcely any trace of animal matter. In this respect there is a considerable difference in various specimens. In some comparative trials made by Professor Buckland, although the proportion of animal matter was greater than in my experiment, he found that these bones contained about one-third part less than the bones from the cavern at Kirkdale.

Is it not therefore probable, that the clay immediately surrounding the bones, which is of a darker colour as well as more tenacious than that in which no bones were found, may have abstracted a large proportion of the animal matter, and be the principal cause of the extremely fragile state of the bones? for they are now so absorbent, that if the largest of them be applied to the surface of the tongue, they adhere so firmly as to support their whole weight. In this, they resemble those bones which were discovered in 1816, and 1820; most of them being as white and fragile as though they had been calcined.

It would appear that the loss of animal matter, and consequent decay or decomposition of fossil bones, depends very much upon the nature of the soil in which they are deposited;

and on its elevation, and different degrees of moisture at different periods; and perhaps, in a great measure on the density or compactness of the bones themselves. There are specimens in the Museum of the College of Surgeons, of sections of teeth of the animal incognitum, or mastodon, from the blue clay on the banks of the Ohio; and of the bear from the caverns at Gaylenreuth, which have retained their animal matter so entirely, as to preserve their form most perfectly, after having been deprived of their earth by means of muriatic acid, while, under other circumstances, teeth and bones of the densest kind, lose their cohesion immediately after being exposed to the air, and becoming dry. This is constantly the case with the tusks, molares, and other bones of the elephant, so frequently found in the yellow sand above the blue clay at Brentford, Ilford, and other situations in the vicinity of the River Thames; which invariably separate into small lozenge-formed or cubic fragments as soon as they become dry.

On immersing the bones of the carnivorous animals in water, more effectually to remove the clay without injuring the surface, they effervesced strongly, and became nearly of a black colour, but recovered their former appearance on drying. A similar effect was produced, but in a less degree as to colour, on the bones of the bovine animals, and of the horse.

It may be worthy of remark, that appearances of disease in fossil bones are of rare occurrence; and I have never yet seen an instance of fracture that had been united during the life of the animal: but among these occur two examples in the metacarpal and metatarsal bones of the bovine animals,

which unequivocally show the effects of ossific inflammation on their surface; (Fig. 1.) and the lower jaw of a young wolf, in which an abscess on each side had produced sinuses, and a considerable alteration in its form and texture.* Fig. 2, and Fig. 3.

All the bones from these caverns which have come under my observation, are clearly referable to animals of known, and still existing genera, as will appear by the following enumeration: but it is a curious circumstance, that, with the exception of the very few belonging to the deer, they all appertained to animals entirely differing from those found in the immediate vicinity in the former instances.

Of the bovine genus, there are specimens of the bony core of the horns belonging to three individuals of different size; (Fig. 4.) all of them remarkably short, conical, and slightly curved, and standing in a nearly horizontal direction from the head. They evidently do not belong to very young animals, and from the appearance of these alone, a very small species would be inferred; but numerous specimens of the teeth, of the os humeri, ulna and radius, os femoris, tibia, os calcis, metacarpus and metatarsus, and phalanges, (Fig. 5.) clearly prove that they belonged to individuals considerably larger

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* On mentioning this circumstance to Professor Buckland, he informed me that he had lately seen in the Collection of Professor Sömmerring of Munich, the skull of a very old hyæna from the caves of Gaylenreuth, in which the incisor and canine teeth, with the jaw containing them, had been entirely torn away, and the occipital and parietal crest dreadfully fractured and perforated, apparently in an affray with some more powerful animal; after which, a healing, and partial renovation of the parts had taken place, and the animal had lived on to mature old age, from the state of its masticating organs.

than the average size of animals of that genus at the present day.

The number of bones collected, afford sufficient grounds for supposing them to have belonged to more than a dozen individuals, varying considerably in their age.

Of smaller ruminants, there are a few portions of the cylindrical bones belonging to one or two individuals, which are too imperfect to admit of being very satisfactorily identified, but apparently are those of a deer; and some others belonging to very young animals in which the epiphyses had not been united, and consequently the bones had not acquired sufficient distinctness of character to allow of our speaking decidedly concerning them; but they have been most probably those of a calf or fawn.

Of the horse, the bones are satisfactorily identified by various specimens of the teeth, the large cylindrical bones, the os calcis, metacarpus and metatarsus, first and second coronary bones, the sesamoid or nut-bone, and particularly by the terminal phalange or coffin-bone of the foot. (Fig. 6.) From the number of these there must have been twelve or more individuals of not less than fourteen hands high; one of the metatarsal bones measuring eleven inches and a half in length. Some of these animals, from the worn state of the teeth, appear to have been very aged.

Of the hyæna, there are bones and teeth which belonged to at least five or six individuals of various ages; some of them equalling the largest of those found at Kirkdale in 1820. Among these, is a part of the right side of the lower jaw, in which remain one of the shedding molar teeth, and

two permanent ones which had not sufficiently advanced in their growth to have protruded through the gum, but are still enclosed within their alveolar cavities. (Fig. 7.) Also part of the right side of the lower jaw of an adult animal, with the teeth in a good state of preservation. This specimen was discovered in the cavern marked B, in Plate VI.

There are likewise detached specimens of the canine teeth and molares of individuals of very large size: and the posterior part of a skull of uncommon magnitude, which corresponds most exactly in form with that of a hyæna, and must undoubtedly have belonged to that animal, but measures twice as much from every determinate point to another, as a recent full grown hyæna's skull. (Fig. 8.)

Of the wolf, there are some bones of several individuals which were found in the cavern marked E, Plate I. There are two large portions of the lower jaw, containing nearly all the teeth in good preservation, and perfectly agreeing in size, in form, and arrangement, with those of a full grown recent animal. (Fig. 10.)

The os humeri also is perfectly similar, and has the rounded aperture through its lower extremity to receive the curved process of the olecranon.

A few very small fragments of shell were found in the situation denoted by the letter H. Plate VII, apparently allied to the genus ostrea; but they are too minute to admit of even that being positively ascertained. A single valve would produce more than all the fragments in question: when applied to the tongue they do not adhere, and their pearly surfaces have all the compactness and lustre of a recent shell.

Since the above was written, Mr. Whidbey has transmitted some additional specimens of the jaws and teeth of the hyæna, the wolf, and the fox, which have been subsequently discovered in the cavern marked E, and from which cavity all the bones of the wolf have been derived. Among these is half of the lower jaw of a hyæna of very superior magnitude to any of those previously discovered, (Fig. 9), and probably has belonged to the large skull before-mentioned.

The jaws of the wolf are of similar dimensions with those before described; but one of them belonged to a very aged individual.

Of the fox, there have been found only a few vertebræ, and two canine teeth from the lower jaw, which correspond perfectly in size and form with those of a recent animal; but are equally fragile and absorbent with those of the other animals.

In a subsequent Letter of Mr. Whidbey to Mr. Barrow, relating to these last mentioned specimens, dated Plymouth, November 9, 1822, he communicates the following additional information:

"These, I think, will be the last bones I shall send you "from these caves, as they are now nearly worked out. The "cave B terminated near where it was first seen: the head "of it was closed over with a body of lime-stone.

"The joints of the rock were not so close but that water "might drop down into the cave; and about these joints "some stalactites were found in small pieces. I have not "seen any thing to encourage the idea that the cavern had a "communication with the surface since the flood; the present state of the quarries shows nothing like it."

EXPLANATION OF THE PLATES.

The figures are all of the natural size.

PLATE VIII.

Fig. 1. A posterior view of part of the right metatarsal bone of the bos, showing the effects of long continued ossific inflammation.

Fig. 2. Inside view of part of the lower jaw of a young wolf, in a diseased state from abscess.

Fig. 3. Outer view of the same jaw.

PLATE IX.

Fig. 4. The bony core of the left horn, and a small part of the skull, seen from behind. It is the middle-sized specimen, and most perfect of the three.

Fig. 5. The lower extremity of the metacarpal bone and the three pair of phalanges of the bos, viewed anteriorly.

PLATE X.

Fig. 6. The two coronary bones and the terminal phalange or coffin-bone of the foot of the horse.

Fig. 7. An outside view of part of the lower jaw of a young hyæna, in which remains one of the temporary or shedding molar teeth, and two permanent ones not perfectly formed, and consequently had not yet cut the gum.

PLATE XI.

Fig. 8. An oblique view of the posterior part of the skull of

a very large hyæna, in which the parietal and occipital crest is greatly extended to afford surface for the attachment of muscles.

Plate XII.

Fig. 9. An outside view of the left side of the lower jaw of an hyæna of very large size, where the teeth are considerably abraded by masticating hard substances.

Fig. 10. An outside view of part of the lower jaw of a wolf, which corresponds most exactly in number, form, and size, with those of a full grown recent animal.

Fig. 1.

Fig. 2.

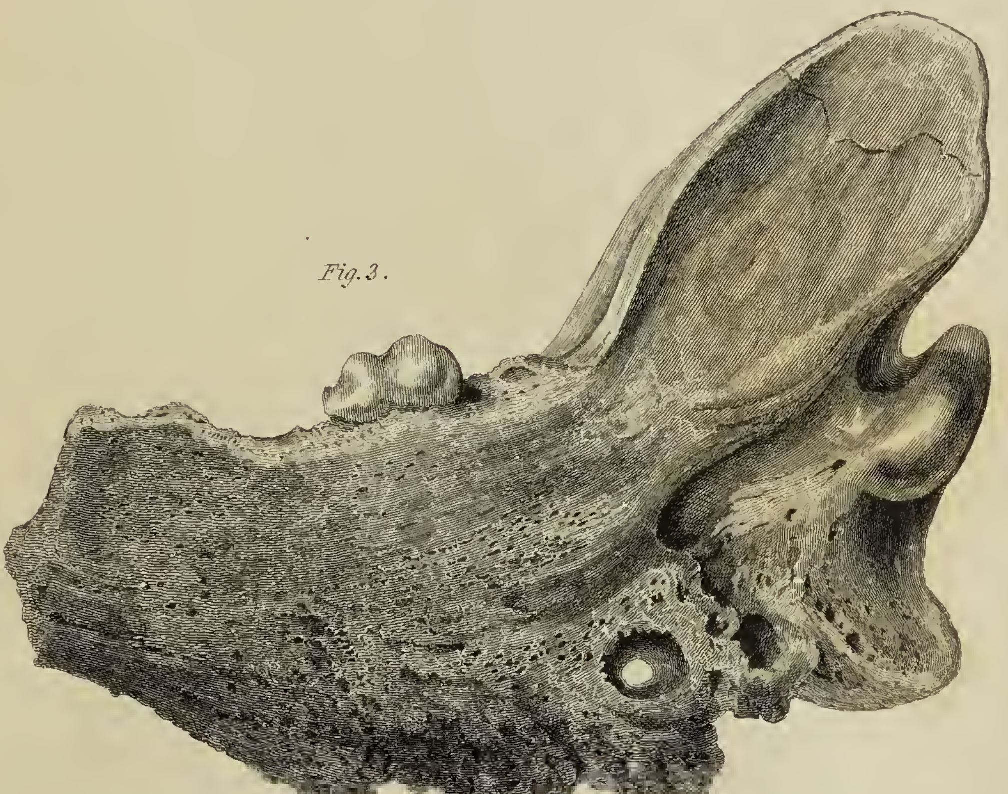


Fig. 3.

Js Basire sculpt.

Fig. 4.

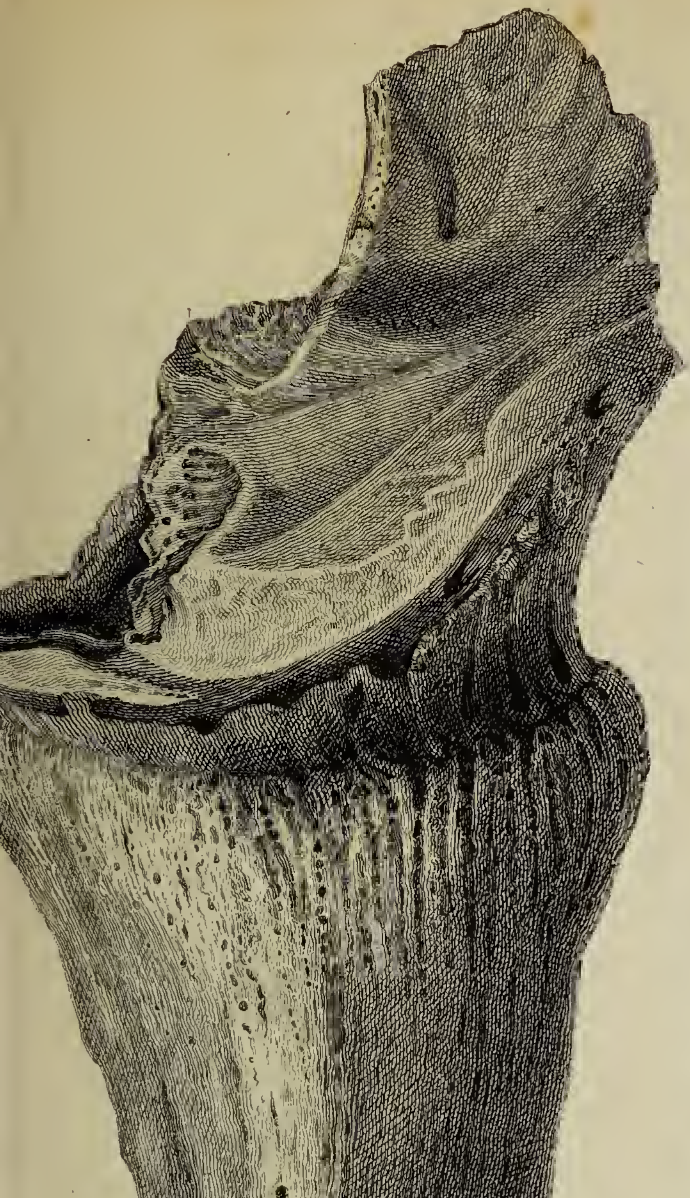


Fig. 5.

J^s Basire, sculp^t.

Fig. 6.

Fig. 7.

delt.

Js. Basire, sculpt.

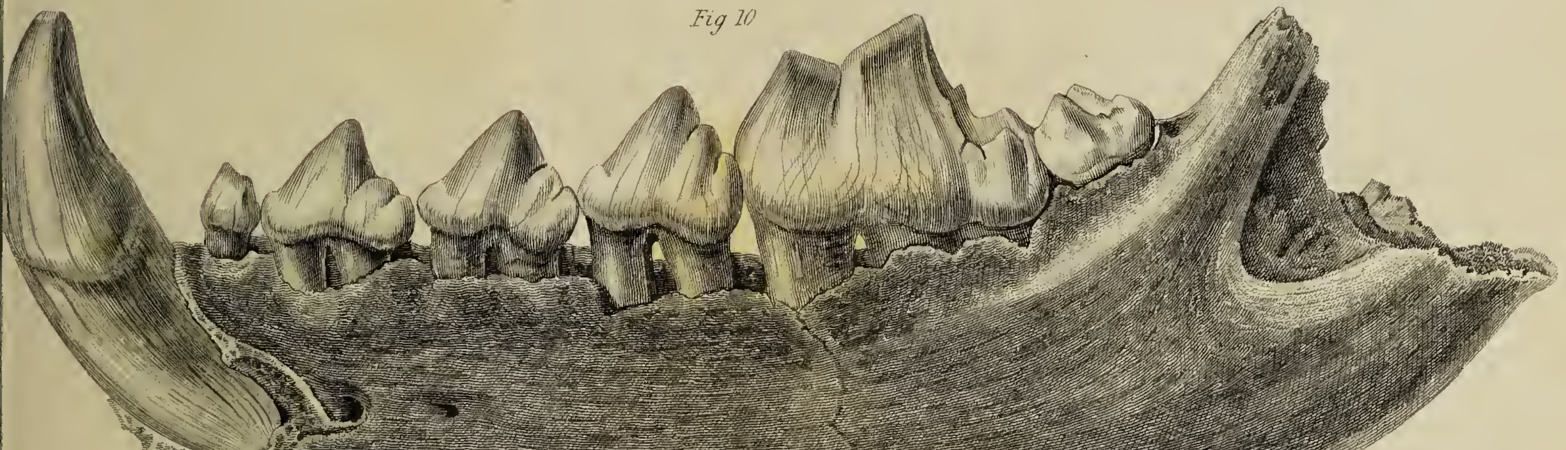
Fig. 8.

del^t.

J^s. Basire, sculp^t.

Fig. 9.

Fig 10



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Js Basire, sculpt

X. *On the Chinese Year.* By J. F. DAVIS, *Esq. F. R. S.*

Read December 19, 1822.

ALL investigation into the Chinese knowledge of astronomy tends only to prove, that before the introduction of that science into the empire, first by the Arabians, and afterwards by the European missionaries, they were wholly ignorant of its principles. It is true that CONFUCIUS has recorded thirty-six eclipses of the sun, the greater number of which have been verified by the calculations of European astronomers: but, as has been very truly observed, the *recording* an eclipse may prove the authenticity of historical annals, while, at the same time, it proves nothing as to the existence of astronomical science. As far as related to the mere *observation* of the sky, the Chinese have, from the earliest periods, been very particular and assiduous. The remark of DU HALDE, that "all these observations are not a little serviceable in ascertaining their chronology," is very true, but they by no means prove (what he sometimes appears desirous to establish), that the Chinese were astronomers.

On this one subject, that singular nation has deviated from its established prejudices and maxims against introducing what is foreign; and that a people so self-sufficient and vain, should at once, in open violation of their general practice, have adopted the science of foreigners, and raised its professors to high dignities, is the strongest possible proof that they had no science of their own.* It even appears

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* The most sensible estimate of the extent of Chinese knowledge has been made by RENAUDOT, in his observations on the accounts of the Arabian travellers.

that they have in former times adopted the very *errors* of European astronomy. The writer of this discovered, in an old Chinese book, the most exact delineation of the Ptolemaic system, with its crystalline orbs, primum mobile, &c. &c. and the earth occupying a conspicuous place in the centre of all. Indeed it is impossible not to smile at the idea of attributing any science to a people whose learned books are filled with such trumpery as the diagrams of Fo-hi, and a hundred other puerilities of the same kind.

There cannot be a doubt that the instruments mentioned by DU HALDE, as having been found by the missionaries on their first entrance into the country, were constructed by the Arabians. His observation, that " the uses of these instruments were written in Chinese characters, with the names of the constellations, which are 28 in number," proves nothing to the contrary. The guns which were cast for the Chinese by Europeans, were always inscribed with Chinese characters; and the ungrateful vanity of that people has invariably led them, when they have borrowed any thing from foreigners, to conceal the debt as much as possible. In proof of this, the writer is able to state the following fact: when Mr. PEARSON made them his invaluable present of the vaccine inoculation, it was accompanied by a small pamphlet in Chinese, containing a few necessary directions as to the use of the virus, and stating the discovery to have been English. An *expurgata* edition of this little book was very soon after published, in which not one word was retained as to its origin, nor any trace by which it could be known that the discovery of vaccination was otherwise than Chinese.

In the accompanying view [Pl. XIII.] of the Chinese year, are marked down, in the outer circle of all, the signs of our


CHINESE YEAR.
January
February
March
April
May
June
July
August
September
October
November
December
1st Moon
2nd Moon
3rd Moon
4th Moon
5th Moon
6th Moon
7th Moon
8th Moon
9th Moon
10th Moon
11th Moon
12th Moon
30°
13½°
6°
17½°
20°
18°
12°
9½°
16°
18°
9½°
22½°
7°
11°
9°
16°
18°
9½°
18°
12½°
15°
11°
16½°

J. Basire sc.

European zodiac, for the sake of comparison; and in the second circle is described *their* zodiac, or the twenty-eight constellations, with the same number of degrees affixed to each, as are given to them in the Chinese books, making 360 in all. It will be observed that they are extremely unequal, the largest, 井 *tsing*, consisting of 30 degrees, and the least, 觜 *tsuy*, of not more than about half a degree. Of these 28 constellations, 角 *Kiŏ*, denoted by Spica Virginis, is considered as the first in order.*

In the circle next to the constellations are noted the Chinese moons, or months, of which 正 *ching* is the 1st, 二 *urh* is the 2nd, &c.: and in the innermost circle are described the 24 *Tsiĕ-ky*, or divisions of half moons, each consisting of about 15 days. Their names have a reference to some prevailing circumstance in each season, as 雨水 *Yu-shuy*, "rain and water," 清明 *tsing-ming*, "clear and bright," 大雪 *ta-siŭ*, "much snow," &c.

The Chinese, as far as the writer of this knows, have no solar year, unless their celebration of a grand festival at the 冬至 *tung-chy*, or winter solstice, may be considered as an observance of its annual limits. Du Halde says, that

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* These constellations are generally found written on the Chinese compass, together with the diagrams of Fo-hi, the *five* elements (viz. fire, water, *wood*, stone, and metal, a division strikingly philosophical) and various other characters used in fortune-telling.

"their (solar) year is composed of 365 days and somewhat less than 6 hours, and from an epocha regulated by the winter solstice, which was the fixed point of their observations, as the 1st degree of Aries is of ours, reckoning from a hundred to a hundred degrees, they calculated the motions of the planets, and adjusted all things by equation tables: some supposed that they received them from the Arabians, who entered with the Tartars into China." He afterwards acknowledges, that "though the Chinese have distinguished the course of the sun into 365 days and 15 minutes, of which we compose one year, they regard more the lunations, than the course of the sun."

The Chinese year, properly considered as such, is in fact a lunar year, consisting of twelve months of twenty-nine and thirty days alternately, with the triennial intercalation of a thirteenth month, to make it correspond more nearly with the sun's course.* It has not been discovered (with any degree of certainty), *why* they fix upon the 15th degree of Aquarius as a rule for regulating the commencement of their lunar year: but they have an annual festival about the recurrence of this period 立春 which bears a considerable resemblance to the deification of the bull Apis; and this resemblance is increased by the connection of both ceremonies with the labours of agriculture, and with the hopes of an abundant season.† This coincidence may serve to fortify the opinions of those who are fond of tracing the Chinese to the Egyptians; although the possibility of such a derivation has been ably disproved by M. de Pauw.

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* I call this intercalation triennial, because that is the nearest approximation; but in fact it is seven times in nineteen years.

† For a detail of the Chinese ceremony, vide Morrison's view of China, p. 109.

An astronomical work, in which the writer of this found the Chinese planisphere, comprised in 12 charts of the heavens, mentions the N. Pole as being 36° above the horizon, and hence he concludes, that what it contains is independent of the astronomy which was afterwards introduced by the European Missionaries, because, as the elevation of the pole at any particular place is the latitude of that place, it is probable the contents of this book were compiled when the Chinese Court and observatory were in Honan, a part of which province is as high as 36° N. It appears from Du Halde, that the astronomical instruments, mentioned above as having been constructed by the *Arabians* for the Chinese, were calculated for 36° latitude.

Mr. Reeves, of Canton, has with much labour compared the modern Chinese planisphere with the European constellations, and his catalogue of stars is printed at the end of Dr. Morrison's Dictionary.

J. F. Davis.

XI. *Experiments for ascertaining the Velocity of Sound, at Madras in the East Indies. By* John Goldingham, *Esq. F. R. S.*

Read February 20, 1823.

The manner in which sound is conveyed, and the theory of its velocity, are too well known, and are to be found in too many scientific works, to need detailed repetition here. The actual rate of its motion, particularly in different states of the atmosphere, does not appear, however, to have been so well agreed upon by Philosophers. A scientific writer* in a standard work, states "That some of the most eminent philosophers, judging that the knowledge of the flight of sound might be of use on various occasions, have been at extraordinary pains and expense to measure the rate at which it moved; and the result of their experiments, particularly of those which were best conducted, is as follows:

1. "That the velocity of sound is the same, whether by sea or by land, in dry or in rainy weather, by day or by night, in winter or summer.

2. "That sound, whether more or less strong, flies with the same swiftness. For, by experiments, a cannon fired with a half-pound charge of powder was heard at about the distance of seventeen miles and a half in the same time after the flash was seen, as it was when fired with a charge of 6lbs.

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* Robertson.

3. " That the times in which sound is heard are proportional to the distance; that is, at a double distance it is heard in twice the time; at a triple distance in thrice the time," &c.

This, however, is not supported in *all* points by the experiments I am about to detail; nor indeed could we expect it would be, from the manner in which sound is conveyed; as this leads us to the conclusion, that the more dense, and the less elastic, the atmosphere, the slower sound would travel.

The velocity of sound has been variously given by different philosophers, as the following table,* originally from the Philosophical Transactions, will tend to show.

	Pedes.	
D. Is. Newton, Eq. Aur.	968	Prin. Ph. Nat. Math. L. 2 Prop. 50.
Nobilis D. Roberts - -	1300	Philos. Transact. No. 209.
Nobilis D. Boyle - -	1200	Essay of Languid Motion, p. 24.
D. Walker - - - -	1338	Philos. Transact. No. 247.
Mersennus - - - -	1474	Balistic Prop. 39.
D. Flamsteed and Halley	1142	
Florentini celebres. - -	1148	Exp. per Acad. del Cimea p. 141.
Galli celebres - - -	1172	Du Hamel Hist. Acad. Reg.

To these may be added the following results of more modern experiments and calculations.

In Chili the thermometer 73,5, barometer 27,44 in. sound was found to travel 1227 feet in a second. By Mr. Millington 1130 feet.

Mr. Richard Van Rees has shown theoretically that the velocity of sound in common air is 341,54 metres in a second.

Mr. Bengenberg at Dusseldorf, by experiment, 333,7 metres, about 2 feet and a half more, it is stated, than the velocity obtained at Paris by experiment.

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* Being a portion of a dissertation by Mr. Derham, who, if I recollect right, makes the velocity the same as Flamsteed and Halley.

A metre, according to the accurate investigations lately made in England, contains 39,37079 English inches, and therefore 341,54 metres will be equal to 1120, and 333,7 to 1094,8 English feet.

" Some curious experiments were made relative to sound by Messrs. DE THURY, MARALDI, and DE LA CAILLE, upon a line 14636 fathoms in length, having the tower of Mount Lheri at one end, and the pyramid of Montmartre at the other extremity, their Observatory was placed between the two objects. The result of their observations was, that sound moves 173 French* fathoms in a second when the air is calm. 2. That sound moves with the same degree of swiftness whether it is strong or weak, an explosion of half a pound of powder discharged in a box, having been heard in the same space of time, as the report of a great gun charged with nearly six pounds of powder. 3. That the motion of sound is uniform, its velocity neither accelerating nor diminishing through the whole course of its progress. 4. That sound travelled at the same rate, whether the gun be pointed perpendicular to the horizon, or *towards* the person who hears the report, or *from* him — by other experiments however, the progress of sound appeared to be impeded by a strong wind." *Dr.* G. GREGORY, *Econ. of Nat.*

The velocity in the foregoing table, stated to be Sir ISAAC NEWTON'S, does not agree with that given by himself, which is 979 feet; this however is not deduced from experiment, but from the theory, no regard being had to the thickness of the solid particles of air, through which sound is propagated. This being allowed for, according to the formula,

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* Or 1106¼ English feet.

brings out the velocity of sound greater in the proportion of ten to nine, or 109 feet more, making the velocity 1088 feet; besides, vapours are dispersed through the air, which being of a different tone and elasticity, do not partake of the motion of the true air, by which sound is propagated; and it is also demonstrated, that the motion of sound will be quicker in such an atmosphere than in an atmosphere of true air, in the ratio of twenty-one to twenty; then the velocity last found being augmented in that proportion, we shall have 1142 feet for the velocity of sound in a second, according to Sir ISAAC NEWTON's theory.

LA PLACE, using the Newtonian formula, which he considers correct, and a theorem which he gives, makes the velocity of sound in a second 345,35 metres (or 1133,06 feet English) the temperature being 43°. The French Academicians, as before mentioned, found the velocity 337,18 metres, or 1106¼ feet English. By experiments of LACAILLE the velocity was 344,42 metres, 1130,1 feet English, but the temperature is not mentioned.

In the above enumeration of the velocity of sound, given by different philosophers, very considerable discordances are observable; the actual reason of which cannot, I imagine, be discovered without the details of the experiments, and these are not in my possession; but probably, a particular examination of the experiments I am about to submit, may furnish a clue for the discovery of the cause of these differences. HALLEY and FLAMSTEED are the only two, whose results agree with the theory; but I am not quite certain whether their results were deduced from theory or experiment. Be this as it may, the conclusion drawn from the experiments

made here agrees, in a very satisfactory manner, with that given by Sir ISAAC NEWTON'S theory, and by the two other celebrated men just named.

Between the years 1793 and 1796 a considerable number of observations were taken by myself, and under my superintendence, at the Observatory, with the view of ascertaining the velocity of sound. Not having the exact distances of the guns from the station when I returned to England, I wrote for farther information upon the subject—which I had not obtained when I quitted Europe again. I therefore did not bring these experiments forward at the time; and having a more elevated station to observe from, by the erection of a new building, and the advantage of corroborating distances, by the trigonometrical survey carrying on under the superintendence of Colonel LAMBTON, I entered upon the course of experiments about to be detailed. The former experiments (those of 1793 and 1796) were made with ARNOLD'S chronometers, as were these now given. In examining works obtained from libraries here, since I closed these experiments, for information relative to the results of like experiments by other observers, I found a letter from Colonel BEAUFOY, in the Annals of Philosophy, addressed a few years ago to Dr. THOMSON; and recommending to be done in England, what, in all the essential points, has been performed here, as will appear by the following extract:

"It has frequently excited my surprise, as well as regret, and in which I am no wise singular, that use has not been made of the admirable Trigonometrical Survey, begun by the late General ROY, and continued with so much ability and attention by Colonel MUDGE and Professor DALBY, to

make experiments on the velocity of sound; and however experiments of this kind may have been neglected, it is hoped that the present Master General of the Ordnance, a near relation of the late scientific Captain PHIPPS, (afterwards Lord MULGRAVE) will, for the purpose of perfecting a branch of science, no less curious than useful, order a series of experiments of this nature to be undertaken, not only in the inland parts of the kingdom, but also on different parts of the Coast." He then mentions that the experiments should be made under different circumstances of the wind and weather, and at different times of the 24 hours, and proceeds to enumerate the stations where the experiments should be made. He recommends that pocket chronometers should be used, "which generally making five beats in two seconds, the velocity of sound could be determined to the fraction of a second;" and concludes by saying, "he has no doubt scientific foreigners would assist our countrymen in finding the time sound is travelling across that part of the Channel, where the shores are visible from each other."

At Fort St. George (Madras) a morning and an evening gun are fired from the ramparts, as is customary in fortified places, the former at day light, and the latter at eight o'clock in the evening. At St. Thomas's Mount, the artillery cantonment, morning and evening guns are also fired, one at day light, and the other at sun set. The Madras Observatory, in latitude 13° 4′ 8″ north, is situated between these; the distance of it from the Fort, about half its distance from the Mount, the Fort being to the N. E. of the Observatory, and the Mount to the S.W. In former years, as I have mentioned before, experiments were made by me for ascertaining

the velocity of sound, but were not brought forward. And a new building,* elevated so as to give a commanding view of the country, particularly of the Mount† and Fort‡, having been erected, I commenced a new series with the morning and evening guns of both places. The experiments with the Mount gun, it will be seen, comprise an interval, which embraces all the varieties of the wind and weather during the revolution of the sun; the interval with the Fort gun is less, in consequence of the morning and evening guns having been fired from different parts of the ramparts, after the date at which the Fort experiments close. All the experiments were made with chronometers, which had 100 beats in 40 seconds, sometimes by three observers, myself and two of the Observatory Bramin assistants, but generally by two: the observers having repaired to the station at the top of the Observatory building, a little before the expected time, and each holding his chronometer so that he could distinctly hear the beats, began to count the instant he saw the flash, and continued counting until he heard the report; the number of beats between the flash and report was then immediately put down upon a slip of paper, by each observer, without communication with the others, and the papers delivered to me for their contents to be registered; the height of the thermometer, barometer, and hygrometer, with the direction of the wind and state of the weather, were also observed at the time, and registered; and in this manner the whole of the

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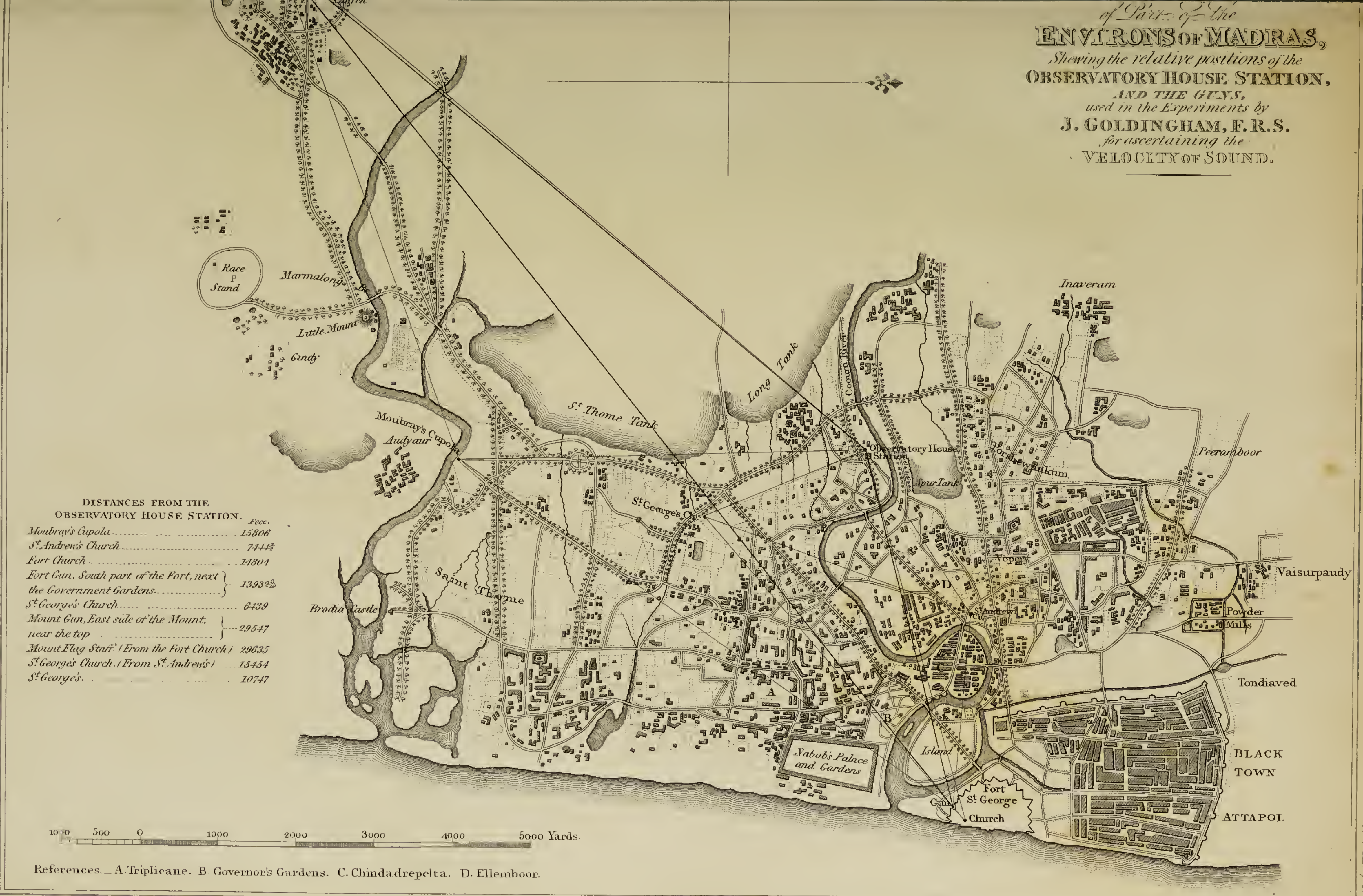
* The station on this building is about 55 feet above the level of the sea, distant in a direct line 4500 yards.

† The Mount gun is about 120 feet above the level of the sea.

‡ The Fort gun is about 30 feet above the level of the sea..



of Part of the

ENVIRONS OF MADRAS,

Shewing the relative positions of the

OBSERVATORY HOUSE STATION,

AND THE GUNS,

used in the Experiments by

J. GOLDINGHAM, F.R.S.

for ascertaining the

VELOCITY OF SOUND.

DISTANCES FROM THE OBSERVATORY HOUSE STATION.

	Feet.
Moubray's Cupola	*15806*
St. Andrew's Church	*7444½*
Fort Church	*14804*
Fort Gun, South part of the Fort, next the Government Gardens	*13932 2/10*
St. George's Church	*6439*
Mount Gun, East side of the Mount, near the top	*29547*
Mount Flag Staff (From the Fort Church)	*29635*
St. George's Church (From St. Andrew's)	*15454*
St. George's	*10747*

References._ A. Triplicane. B. Governor's Gardens. C. Chindadrepelta. D. Ellemboor.

J. Basire, sc.

experiments were made. The situations of the guns with respect to the station from which the observations were taken, was very favourable, being in the direction, one of N. E. and the other of the S.W. monsoons—with the southerly wind and sea breeze, (both which prevail at certain seasons of the year,) blowing between the two. The guns used were 24 pounders, charged with 8lbs. of powder, and both pointed, not exactly towards the station, but in a direction not far from it.

The distances were ascertained with great care: first, by a survey made for the purpose, a base having been measured, and the angles taken with a grand circular instrument, similar to that used on the trigonometrical surveys.* Secondly, by using two or three of Colonel LAMBTON's distances and bearings found by the trigonometrical survey.

The results were thus deduced, and verified in different ways; and I have reason to think that the distances of the guns from the Observatory station are very accurately given. The mean of 12 results made the distance of the Mount Gun from the station 29547 feet; and the mean of 6 results, gave the distance of the Fort Gun from the station 13932,3 feet. The map† will show the exact relative positions and distances of the points, and the face of the country over which the sound travelled.

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* I have not given the details of the survey, as that would swell the Paper to an inconvenient size: the base, however, was measured with great care twice, and generally six observations were taken for finding each angle, each observation differing very little from the other.

† The angles and distances were protracted, and the map of the intermediate country filled up from the best surveys, under the superintendance of E. LAKE, Esq. of the Madras Engineers, my son-in-law.

We see, as I before remarked, that the distance of one gun from the station is nearly double that of the other, and this will be found an advantage, in showing whether sound travels equally during its progress.

The experiments are given in the subjoined Tables.

Table I. Contains the experiments of each day with the Mount gun, together with the state of the atmosphere and the direction of the wind at the time of observation: the titles at the heads of the columns render a particular explanation unnecessary—the number of observers is stated in the third column, and the mean of their observations in the ninth.

Table II. Contains the mean of observations of each day, when the air was calm.

Table III. The mean of observations of three days, when the wind was in the S. E. quarter.

Table IV. The mean of observations of three days, when the wind was in the N. E. quarter.

Table V. The mean of observations of three days, when the wind was S.W. by W. or N.W.

Table VI. The experiments with the Fort gun, arranged as those in Table I., with the Mount gun.

Tables VII., VIII., IX., and X. The experiments with the Fort gun arranged according to the state of the wind, as in the former Tables of experiments with the Mount gun.

Table XI. Shows the mean motion of sound for each month at the Madras Observatory, as found by the experiments, at the mean height of the thermometer, barometer, and hygrometer, given in the table.

Upon a cursory inspection of Tables I. and IV., it will be seen that the motion of sound varies under different states of

the atmosphere and weather: that according to the first table, sound at one time has been as long as 27,6 seconds in travelling from the Mount to the Observatory station; and at another time only 24,8 seconds; the distance being 29547 feet. In the first case, therefore, the velocity of sound was only about 1078 feet in a second; while in the other, its velocity was nearly $1191\frac{1}{2}$ feet. The extremes in Table VI., show a still greater difference. This proves the necessity for making experiments during a long interval, in order to obtain an accurate general result.

In Tables II. and VII. we find, as the thermometer rose, the atmosphere at the same time decreasing in density and increasing in its elasticity, that the sound moved with greater rapidity.

That with the wind in the SE. quarter the velocity was considerably increased, both from the Mount and Fort; more, however, in proportion, as might be expected, from the former than the latter.

That with the wind at NE. the sound from the Fort gun travelled with a greater, and from the Mount gun with a less velocity, than when the wind was in any other direction; that wind being favourable for increasing the velocity from the Fort, and unfavourable from the Mount: the full effect of the wind, however, is not to be ascertained by this table alone, as the thermometer during the time the NE. wind prevails is comparatively low, and the barometer high; both which, as will have been seen by inspection of the tables, occasion the sound to travel slower than ordinary.

The wind SW. W. and NW. the velocity from the Mount was accelerated, and that from the Fort retarded; but not in

the degree that would have taken place had the thermometer, barometer, and hygrometer, remained the same as in the NE. monsoon; but having been different, the velocity was accelerated from both guns on this account, in like manner as it was retarded in the NE. monsoon.

The following are the results deduced from the experiments in the different tables. I shall first give the general results from Table I. and VI.

	Mean height of Barom.	Thermometer.	Hygrometer.	Seconds.	Distance.	Velocity in a Second.
Tab.	Inch.	°	°		Feet.	Feet.
I.	29,992	84,11	19	25,869	29,547	1142,18

Or almost precisely the same as the velocity by the theory.

	Barometer.	Thermometer.	Hygrometer.	Seconds.	Distance.	Velocity in a Second.
Tab.	Inch.	°	Dry.		Feet.	Feet.
VI.	30,065	80,47	11,4	12,306	13932,3	1132,14

Here we find a difference from the former general result by the observations with the Mount gun; the reason of which appears to be, that I could not, as I have before stated, carry on the observations during at least a complete revolution of the changes in the atmosphere; and that this is the reason I shall now endeavour to show. The interval wanting is between the 28th of March and the 16th of July. Had this interval been wanting in the experiments with the Mount gun, there would have been a difference of 0,237 seconds in the mean result; for the mean of the experiments in this

interval is 25″,632, and the mean of the whole 25″,869, making the difference just mentioned.

Now 25″,869+0″,237=26″,106, which would have been the mean number of seconds had the observations with the Mount gun been continued during the same interval only as the experiments with the Fort gun. Then 26″,106 : 0″,237 : : 12″,306 (the mean of the Fort observations) : 0″,112. Now 12″,306—0″,112=12″,194, which would have been the general mean of the experiments with the Fort gun, had the same been continued as long as the experiments with the Mount gun. Then the distance 13932,3 feet, divided by 12,194, will give 1142,5 for the motion of sound by the experiments with the Fort gun thus brought on; and this also agrees, within a fraction of a foot, with the velocity according to Sir Isaac Newton; and with the results by the two other celebrated philosophers before named. Feet

We then have by the Mount gun 1142,18 for the velocity.
And by the Fort gun - - - 1142, 5.

The mean is 1142,34, or very nearly the velocity above alluded to. Nothing could be more satisfactory than this general result;* and it may be presumed, that the other results in different states of the atmosphere are equally to be depended upon.

The velocity also by the Fort gun, which, it will be recollected, is little more than half the distance of the Mount gun from the station, shows that sound travels equally during its progress.

In the N.E. monsoon, the sound was very indistinct at times; this however does not appear to have sensibly affected

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* The results by the Mount gun may however be taken as the standard.

its motion. The French academicians indeed proved, as I have mentioned before, that this made no difference in the velocity.

I shall now proceed to the conclusions from the other Tables; and first, those of the experiments with the Mount gun.

	Barometer	Thermometer.	Hygrometer.	Wind.	Seconds.	Distance.	Velocity in a second.
Table	Inches.	°	°		″	Feet.	Feet.
II.	29,990	83,95	20,31	Calm	25,712	29,547	1149,2
III.	29,972	85, 5	19,96	SE	25,754		1147,2
IV.	30,113	81, 7	10, 9	NE	26,812		1102,0
V.	29,934	85, 1	26,	SW.W.&NW	25,374		1164,4

Secondly. The experiments with the Fort gun.

Table							
VII.	30,111	79, 3	11,85	Calm	12,313	13932,3	1131,5
VIII.	30,023	82, 3	14, 6	SE	12,231		1139,1
IX.	30,131	78, 6	7,33	NE	12,340		1129,0
X.	29,979	81, 9	11,41	SW.W.&NW	12, 46		1118,1

The results in these Tables, like the separate observations, show the necessity of making a series of experiments long continued, in order to obtain the correct general rate at which sound travels; and this may afford a clue, as I observed in the first part of this paper, for discovering the cause of the differences in the results by the authorities there named: it is difficult, undoubtedly, to ascertain the distance of two stations, one far from the other, to the nearest foot; but errors of many feet in this respect, would make but a small difference in the velocity in a second found by experiment, when

the gun and station were even at a moderate distance ;* we must therefore be led to conclude, that these differences have chiefly arisen from the experiments having been made during a limited period only, and at unfavourable times for obtaining a mean result, instead of the interval which appears by these experiments to be necessary.

A particular examination of the Tables and results, will show the difficulty of ascertaining what proportion of the differences should be allowed to each of the instruments used for finding the state of atmosphere, exclusive of the effects of the wind.

During the calms, we might expect that the proportional parts to be allowed for the difference in the thermometer, barometer, and hygrometer, might be found with some degree of accuracy ; the discrepances, however, are very considerable. Comparing the results of Tables II. and VII. we find the barometer 0,121 lower, the thermometer 4°,6 higher, and the air about $8\frac{1}{2}$ more dry by the former Table than by the latter, while the velocity in a second is only 17,7 feet greater by one Table than the other.

We give however in addition the following results taken from the Tables of calms, and arranged according to the different heights of the thermometer and barometer. These results may assist us in coming to some conclusion upon this part of the subject.

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* For example, a difference of about twenty-six feet in the distance, between the Observatory station and the Mount gun, would make only about a foot difference in the velocity in a second.

* *Experiments with the Mount gun.*

Barometer.	Thermometer.	Hygrometer.	Seconds.	Distance.	Velocity in a Second.
Inches.	°	°		Feet.	Feet.
30,109	88,13	26,4	25,97	29,547	1137,7
29,889	88,	28,4	25,45		1160,9
30,140	77,16	11,5	26, 3		1123,4
30,089	81, 3	11,3	26,40		1119,2
29,915	84,96	20,3	25,81		1144,7
29, 93	82,12	16,0	25,91		1140,3
30,046	82, 9	18,9	25,75		1146,5

With the Fort gun.

30,163	86, 3	23,8	12,27	13932,3	1135,5
30,135	74, 1	13,8	12,72		1095,3
30,063	80,76	8,8	12,11		1150,5
30,147	77, 5	10,8	12,37		1126,3
29,943	82,25	15,	12,15		1146,7
30,078	82, 4	10,4	12,35		1128,1

Where the changes are so numerous and so frequent as in the atmosphere of the earth, we cannot expect that our imperfect instruments will be of a construction sufficiently delicate to show accurately every alteration that may affect the motion of the pulses of the air; but by various comparisons and combinations of the results, we may hope to arrive at general conclusions, somewhat approaching the truth.

Now, by numerous combinations of the observations just given, when the air was calm, we are led to conclude: first,

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* These are deduced from 100 observations.

that for each degree of the thermometer 1,2 feet may be allowed in the velocity of sound for a second ; for each degree of the hygrometer 1,4 ; and for one-tenth of an inch of the barometer* 9,2 feet. Then taking these numbers as the basis of the comparison, we find the mean difference of the velocity between a calm, and in a moderate breeze of wind, to be nearly 10 feet in a second. And by comparing other results together, a difference of about 21¼ feet in a second, or 1275 in a minute is found between, the wind being in the direction of the motion of sound, or opposed to it.

Before I conclude these introductory observations, and explanations of the experiments, it may be proper to refer more particularly to Table XI., containing the mean motion of sound for each month of the year, by the experiments with the Mount gun, according to the state of the atmosphere indicated by the different instruments ; and to the prevailing monsoons, which may be considered to be the same, during the same months, every year ; full information respecting which is given in the former Tables. On examining this Table, it is rather curious to observe how regularly the mean velocity proceeds to a maximum about the middle of the year, and afterwards retraces its steps ; giving us a velocity in one case 1164 feet in a second, and in the other of only 1099 feet. This regularity would, no doubt, be still greater with the mean of the observations of several years.

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* The rise and fall of the barometer is very limited in this country, as will be seen by an examination of the Tables. A sudden fall of 0,3 inch indicates a gale of wind.

J. GOLDINGHAM.

Madras, 31st May, 1822.

Experiments for ascertaining the motion of Sound, with a gun placed on St. Thomas's Mount, station at the top of the Madras Observatory House.

Table I.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds		
1820.				h.	Inches.	°	Dry.		″		
July	14	1	E	6	29,878	84,8	22	62,0	24,8	SE	Cloudy.
	15	1	E	6	29,910	83,5	23	62,0	24,8	SE	Cloudy and rain.
	16	1	E	6	29,900	83,5	18	64	25,6	Calm	Cloudy and rain.
	17	1	E	6	29,910	82,3	15	63	25,2	SE	Cloudy.
	18	1	E	6	29,842	84,3	17	64	25,6	Calm	Thin haze.
	19	1	M	5	29,870	79,8	12	62	24,8	Light NE	Cloudy and rain.
		1	E	6	29,865	83,3	14	63	25,2	SE	Hazy.
	20	1	E	6	29,855	84,0	15	63	25,2	SE	Clear.
	21	1	E	6	29,870	83,5	15	64	25,6	SE	Hazy.
	22	1	M	5	29,920	80,0	12	64	25,6	Land	Cloudy.
		1	E	6	29,900	80,4	14	62	24,8	SE	Hazy.
	23	1	M	5	29,920	80,5	14	63	25,2	Land	Cloudy.
		1	E	6	29,928	83,3	16	64	25,6	Calm	Cloudy.
	24	1	M	5	29,920	80,0	14	64	25,6	W	Hazy.
		1	E	6	29,915	83,3	16	63	25,2	SE	Cloudy.
	25	1	M	5	29,955	80,2	14	62	24,8	W	Cloudy.
		1	E	6	29,925	84,5	20	63	25,2	SE	Hazy.
	26	1	M	5	30,045	80,0	16	65	26,0	Light W	Hazy.
		1	E	6	30,018	87,0	24	63	25,2	Calm	Thin haze.
	27	1	E	6	30,048	83,3	20	63	25,2	SE	Hazy.
	28	1	M	5	30,055	80,7	15	65	26,0	Land	Cloudy.
		1	E	6	30,020	80,4	11	63	25,2	Light SE	Cloudy.
	29	1	M	5	30,020	79,8	9	65	26,0	SE	Clear.
		1	E	6	30,028	81,8	7	63	25,2	Fresh SW	Cloudy.
	30	1	M	5	30,040	77,8	7	64	25,6	Land	Clear.
		1	E	6	30,000	83,0	8	64	25,6	SE by E	Clear.
	31	1	M	5	30,025	81,0	8	62	24,8	Light SW	Clear.
		1	E	6	29,966	84,0	10	64	25,6	Light NE	Cloudy; some rain, thunder and lightning.
Aug.	1	1	E	6	29,965	81,0	7	62	24,8	Calm	Clear.
	2	1	E	6	29,968	81,8	4	63	25,2	NW	Cloudy.
	3	2	M	5	30,020	80,2	4	63,5	25,4	W	Cloudy.
		1	E	6	29,975	82,5	6	63,0	25,2	Light SE	Thin haze.
	4	1	E	6	30,000	82,0	9	64	25,6	SE	Clear.
	5	2	M	5	30,030	80,0	7	64	25,6	Calm	Cloudy, and some rain.
		1	E	6	29,968	82,0	7	63	25,2	SE	Hazy.
	6	1	E	6	29,955	83,0	10	63	25,2	SE	Thick haze.
	7	2	M	5	30,000	80,5	9	64	25,6	W	Cloudy.

Table I. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1820.				h.	Inches	°	Dry.		″		
Aug.	7	1	E	6	29,988	81,5	11	64	25,6	SE	Cloudy.
	8	1	M	5	30,015	80,2	9	64	25,6	W	Cloudy.
	19	2	E	6	29,800	88,0	20	61, 5	24,6	W	Hazy.
	20	2	E	6	29,855	85,5	16	63, 5	25,4	SE	Hazy.
	21	1	M	5	29,960	83,0	14	62	24,8	Light land	Clear.
		2	E	6	29,935	85,6	18	63	25,2	SE	Clear.
	22	1	E	6	29,965	87,0	19	64	25,6	SE	Hazy; lightning and thunder.
	23	2	E	6	29,956	85,5	18	63, 5	25,4	SE	Hazy.
	24	1	M	5	29,998	83,0	15	64	25,6	Land	Cloudy.
		1	E	6	29,945	82,6	14	63	25,2	SW	Cloudy and rain; lightning with thunder.
	25	2	E	6	29,955	87,5	19	63, 5	25,4	Calm	Hazy.
	27	2	E	6	29,920	88,5	25	64, 5	25,8	E	Hazy.
	28	2	M	5	29,965	82,2	20	63, 5	25,4	Land	Clear.
		1	E	6	29,900	85,5	24	64	25,6	NW	Rain.
	29	2	M	5	29,944	82,8	19	63, 5	25,4	Land	Cloudy and lightning.
		1	E	6	29,915	89,5	25	64	25,6	Land	Hazy.
	30	1	E	6	29,957	86,5	20	64	25,6	SE	Clear.
	31	1	E	6	29,925	86,5	19	65, 5	26,2	E breeze	Clear.
Sept.	1	1	E	6	29,900	88,2	22	64	25,6	ESE	Hazy.
	2	1	E	6	29,930	86,5	20	63	25,2	Light SE	Hazy.
	3	2	M	5	29,975	81,5	19	63	25,2	Fresh WSW	Cloudy; lightning in the NE quarter.
		2	E	6	29,935	87,5	18	64	25,6	W by N	Cloudy; lightning to the westward.
	4	2	E	6	29,945	85,2	17	64, 5	25,8	SE by E	Clear; lightning in the SW quarter.
	5	2	M	5	29,970	84,0	15	63, 5	25,4	W by S	Clear.
		1	E	6	29,915	85,2	16	63	25,2	SE	Clear; and lightning at a distance.
	6	1	E	6	29,925	84,5	17	65	26,0	SE by E	Hazy.
	7	2	E	6	29,930	86,0	16	63, 5	25,4	SE by S	Hazy.
	8	1	M	5	29,976	83,2	17	63	25,2	SE	Hazy.
	9	2	E	6	29,945	84.5	19	65	26,0	SE	Clear.
	10	1	–	–	29,948	85,5	18	63	25,2	ESE	Clear.
	11	2	–	–	30,100	84,5	16	64	25,6	SE by E	Cloudy
	12	1	–	–	30,000	82,5	15	65	26,0	NE by E	Cloudy, and some rain.
	13	2	–	–	29,945	80,0	13	63	25,2	SE by S	Cloudy and rain.
	14	2	–	–	29,910	83,0	7	63	25,2	SE by E	Clear.
	15	2	–	–	29,945	83,5	16	64	25,6	ESE	Clear.
	16	2	–	–	30,045	83,5	14	64	25,6	Calm	Cloudy.
	17	1	–	–	30,045	84,5	16	64	25,6	SE by E	Clear.
	18	2	–	–	30,015	84,4	14	64	25,6	SE by E	Cloudy.
	19	1	–	–	30,045	86,0	19	64	25,6	SE	Hazy,
	21	2	–	–	29,950	82,5	13	64, 5	25,8	Calm	Cloudy.
	22	2	–	–	29,956	84,8	14	64	25,6	SE by E	Hazy.
	23	2	–	–	30,020	87,0	23	64, 5	25,8	Calm	Hazy.
	24	2	–	–	30,100	88,5	27	64, 0	25,6	Calm	Hazy.
	25	2	–	–	30,055	86,5	21	64, 0	25,6	SE by E	Hazy.
	26	2	–	–	30,005	84,0	15	64, 5	25,8	SE	Hazy.
	27	2	–	–	30,056	87,0	19	64,25	25,7	SE by S	Clear.
	28	1	–	–	30,065	84,5	19	63, 5	25,4	SW light	Hazy.
	29	2	–	–	30,160	88,5	15	64,25	25,7	NW	Clear.

Table I. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1820.				h.	Inches.	°	Dry.		″		
Sept.	30	2	E	6	30,173	89,2	22	64,25	25,7	Calm	Clear.
Oct.	1	2	–	–	30,200	89,2	27	64, 4	25,8	Calm	Clear.
	2	2	–	–	30,238	91,0	34	64,85	25,9	Calm	Hazy.
	3	2	–	–	30,200	87,8	29	65	26,0	E by N	Thin haze.
	4	1	–	–	30,160	86,6	27	64	25,6	Calm	Hazy.
	5	1	–	–	30,045	86,5	20	64	25,6	SE by S	Hazy.
	6	1	–	–	30,050	86,5	19	64	25,6	SE	Clear.
	7	1	–	–	30,100	86,0	22	64	25,6	SE	Cloudy.
	8	1	–	–	30,088	84,5	22	64, 5	25,8	SE	Hazy.
	9	1	–	–	30,088	84,2	20	64, 0	25,6	SE	Clear.
	10	1	–	–	30,045	85,8	20	64	25,6	SE	Clear.
	11	1	–	–	30,065	83,5	20	64, 5	25,8	SE by S	Clear.
	12	1	–	–	30,045	85,5	21	65, 0	26,0	SE by E	Clear; sound indistinct.
	13	1	–	–	30,065	86,2	20	64, 5	25,8	NE	Clear.
	14	1	–	–	30,075	86,5	22	66	26,4	E	Clear.
	15	1	–	–	30,066	86,4	21	66, 5	26,6	E	Clear.
							Damp				
	20	1	–	–	30,008	77,5	2	68	27,2	NE	Cloudy.
	21	1	–	–	30,085	81,5	2	67, 5	27,0	NE	Clear.
	22	1	–	–	30,055	83,4	0	65, 5	26,2	NE by N	Cloudy; distant thunder.
	24	1	–	–	30,078	82,4	2	66, 5	26,6	NE	Hazy.
							Dry				
	26	1	–	–	30,110	84,0	14	66	26,4	NE by E	Clear.
	28	2	–	–	30,128	83,2	13	66, 5	26,6	NE	Hazy.
							Damp				
	31	2	–	–	30,082	79,8	3	67, 0	26,8	NE light	Cloudy.
Nov.	1	2	–	–	30,072	83,0	4	66	26,4	ENE	Hazy.
	2	2	–	–	30,110	84,2	3,5	66, 5	26,6	NE	Cloudy.
							Dry				
	3	2	–	–	30,115	84,5	3	67, 7	27,1	E	Clear.
	4	2	–	–	30,100	84,5	6	65	26,0	E by S	Clear.
	5	1	–	–	30,135	84,0	11	67	26,8	EN	Clear.
	6	2	–	–	30,140	83,0	15	66	26,4	ESE	Clear.
	7	2	M	5	30,170	77,4	11	65, 5	26,2	S by E	Clear.
		2	E	6	30,168	83,0	15,5	66, 0	26,4	ESE	Clear.
	8	2	M	5	30,188	76,0	12	65, 0	26,0	Calm	Clear; sound indistinct.
		2	E	6	30,178	83,0	17	65,75	26,3	E by N	Clear.
	9	2	E	6	30,200	83,0	14,5	66, 5	26,6	ENE by E	Clear; sound indistinct.
	10	2	M	5	30,178	79,6	11	66,75	26,7	NE	Clear; ditto.
		2	E	6	30,175	83,2	11,5	67, 0	26,8	NE	Clear.
	12	1	–	–	30,220	83,5	14	66, 5	26,6	E by N	Cloudy.
	14	1	–	–	30,134	81,0	1,5	68, 5	27,4	NE	Cloudy.
	15	1	–	–	30,125	79,4	2	68, 5	27,4	NE	Rather cloudy; sound indistinct.
	17	1	–	–	30,165	83,0	14	66, 5	26,6	NE	Hazy.
	18	1	–	–	30,115	79,4	5	67, 5	27,0	Light NE	Hazy.
	19	2	–	–	30,125	81,8	5	67,25	26,9	NE	Clear.
	21	2	–	–	30,110	80,5	11	68,25	27,3	NNE	Clear.
	23	1	–	–	30,076	80,0	12	69	27,6	NE	Clear.

Table I. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1820.				h.	Inches.	°	Dry.		''		
Nov.	24	1	E	6	30,128	80,7	15	68, 5	27,4	NE	Clear; sound very indistinct.
	27	1	–	–	30,135	73,4	14	68	27,2	NE	Clear.
							Damp				
Dec.	4	2	–	–	30,015	75,6	9,5	65,25	26,1	SW by N	Clear.
	5	2	–	–	30,068	79,4	3,0	65, 0	26,0	SE	Clear.
	6	2	–	–	30,075	81,0	6,5	66, 5	26,6	E	Clear.
	7	2	–	–	30,085	80,4	3,5	67,25	26,9	NNE	Clear; sound very indistinct.
							Dry				
	10	2	–	–	30,106	79,3	7,0	68,25	27,3	ENE	Hazy.
	12	2	–	–	30,125	78,8	11,5	68	27,2	N	Cloudy.
	13	1	–	–	30,090	79,0	0,5	67, 5	27,0	NE	Cloudy.
							Damp				
	14	2	M	5	30,074	77,0	1,5	67,25	26,9	NE	Clear.
							Dry				
	15	2	E	6	30,108	81,0	4	67	26,8	E by N	Cloudy.
	16	1	–	–	30,120	81,0	9	67, 5	27,0	NE	Clear.
	17	1	M	5	30,122	77,2	2	66, 5	26,6	NW light	Ditto.
		1	E	6	30,112	80,8	7	68, 5	27,4	NE	Ditto.
	18	1	M	5	30,120	77,0	5	68	27,2	NW	Clear and dew.
	19	1	E	6	30,110	79,5	16	67, 5	27,0	NE	Hazy.
							Damp				
	26	2	–	–	30,028	81,6	9	66,25	26,5	NE	Clear.
1821.	27	1	–	–	30,038	81,5	6	67	26,8	ENE	Hazy.
Jan.	8	1	–	–	30,165	76,2	7	68	27,2	NE light	Cloudy.
	9	2	–	–	30,200	79,2	2	68, 5	27,4	NE	Clear.
							Dry				
	10	2	–	–	30,168	79,0	1	67, 0	26,8	NE	Ditto.
	12	2	–	–	30,220	82,0	9	67, 5	27,0	Calm	Ditto.
	16	1	M	5	30,155	76,2	6	66, 5	26,6	Calm	Ditto.
		2	E	6	30,155	80,0	8,5	67, 0	26,8	Calm	Hazy.
	17	1	M	5	30,175	75,0	7,5	66, 0	26,4	Calm	Clear.
	19	2	E	6	30,155	80,0	2	68, 5	27,3	East	Cloudy.
	21	1	M	5	30,138	76,5	5,5	67, 0	26,8	NE	Clear.
		2	E	6	30,138	79,5	12	67, 5	27,0	E	Ditto.
	22	1	–	–	30,130	80,5	9	66, 5	26,6	NE	Ditto.
	23	2	–	–	30,110	80,6	11,5	66,75	26,7	NE	Ditto.
	24	2	–	–	30,055	80,0	12	66,75	26,7	Calm	Ditto.
	25	2	–	–	30,088	80,5	10	67,75	26,7	Calm	Ditto.
	26	2	–	–	30,100	79,0	10	68, 5	27,4	NE	Cloudy.
	28	1	–	–	30,050	80,5	9,5	67	26,8	Calm	Clear.
	29	1	M	5	30,058	75,2	8,5	66, 5	26,6	Calm	Ditto.
		2	E	6	30,048	81,0	9,5	66,25	26,4	Calm	Clear.
	30	1	–	–	30,048	81,0	9,5	66,25	26,5	Calm	Ditto.
Feb.	1	1	M	5	30,000	74,5	8,5	65	26,0	Calm	Hazy; dew
	2	1	E	6	30,062	81,0	13	67, 5	27,0	NE	Clear.
	3	2	–	–	30,105	80,5	13	67	26,8	E	Clear; sound indistinct.
	4	1	–	–	30,148	80,0	12	68	27,2	NE	Ditto.
		1	M	5	30,110	76,2	11	68	27,2	NE	Ditto.
	5	1	E	6	30,130	80,6	13	67	26,8	Light NE	Ditto.

Table I. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1821.				h.	Inches.	°	Dry.		″		
Feb.	6	1	E	6	30,100	78,0	12,5	66, 5	26,6	NE	Clear; sound indistinct.
		1	M	5	30,100	72,0	12,5	66, 5	26,6	Calm	Ditto.
	7	2	E	6	30,144	80,5	14	66	26,4	Calm	Ditto.
	8	2	–	–	30,135	79,2	29	64,25	25,7	SE	Ditto.
		1	M	5	30,155	72,5	12	65	26,0	NW	Ditto.
	9	1	E	6	30,138	80,2	25	65	26,0	SE light	Ditto.
	10	2	–	–	30,165	79,4	21	65,75	26,3	E	Ditto.
	11	2	–	–	30,215	77,0	16	65, 5	26,2	Calm	Ditto.
	12	2	–	–	30,185	80,5	12,5	66,75	26,7	NE	Ditto.
	13	2	–	–	30,218	80,0	13	67, 0	26,8	Light NE	Ditto.
	14	2	–	–	30,155	80,5	14	66,25	26,5	E by N	Ditto.
	15	2	–	–	30,188	80,0	13	66,75	26,7	E	Ditto.
	16	1	M	5	30,188	75,5	13	66	26,4	Calm	Ditto.
	17	1	E	6	30,168	79,2	15,6	66	26,4	Light SE	Hazy.
	18	1	–	–	30,090	79,0	16	66	26,4	Calm	Clear.
		1	M	5	30,165	73,5	14,5	66	26,4	Calm	Ditto.
	19	2	E	6	30,100	79,5	14	65, 5	26,2	SE	Ditto.
	20	2	–	–	30,125	80,3	14	66	26,4	ESE	Ditto.
	21	2	–	–	30,135	81,0	14	65, 5	26,2	SE	Ditto.
	22	2	–	–	30,115	70,8	14	66,25	26,5	SE by E	Ditto.
	23	2	–	–	30,130	81,6	16	65,75	26,3	E by S	Ditto.
	24	1	–	–	30,120	82,0	16	66	26,4	SE	Ditto.
	25	2	–	–	30,110	81,6	15	65,75	26,3	SE by E	Ditto.
	26	2	–	–	30,100	82,0	14,5	66, 5	26,6	SE	Ditto.
	27	2	–	–	30,036	83,0	14	65, 5	26,2	SE	Ditto.
	28	2	–	–	30,015	81,4	14,5	65,25	26,1	SE	Ditto.
March	1	1	–	–	30,060	82,5	14,5	66	26,4	SE	Hazy.
	2	2	–	–	30,155	82,0	16	65, 5	26,2	SE	Clear.
	3	2	E	–	30,120	82,0	17	65,25	26,1	SE	Ditto.
	4	1	–	–	30,045	79,2	20	65, 0	26,0	SE	Ditto.
	5	2	–	–	30,065	81,5	18,5	65,25	26,1	SE	Ditto.
	6	2	–	–	30,135	82,0	20	65, 5	26,2	SE	Somewhat hazy.
	7	2	–	–	30,125	80,8	19	65,75	26,3	SE	Clear.
		1	M	5	30,125	75,5	19	66	26,4	Light SE	Ditto.
	8	1	E	6	30,110	82,5	19	66	26,4	SE by E	Ditto.
	9	1	–	–	30,105	80,0	14	66, 5	26,6	SE	Ditto.
	11	1	–	–	30,110	82,0	13	64, 5	25,8	SE	Cloudy, and rain.
	12	1	–	–	30,000	82,5	13	65, 0	26,0	SE	Clear.
	13	2	–	–	30,048	83,0	13,5	65,25	26,1	SE	Ditto.
	14	1	–	–	30,115	82,5	14	64, 5	25,8	SE	Ditto.
		1	M	5	30,100	80,4	11,5	65	26	SE	Ditto.
	15	1	M	–	30,125	81,4	14	64	25,6	Light SW	Ditto.
	16	2	E	6	29,960	82,5	13	64, 5	25,8	SE	Ditto.
	19	2	–	–	29,948	83,4	12	64, 5	25,8	SE	Ditto.
	20	1	–	–	30,045	84,0	12	65	26	SE	Cloudy.
	22	1	–	–	29,975	84,0	12,5	65	26	SE	Clear.
	23	1	–	–	29,982	83,8	13	64	25,6	SE	Ditto.
	24	2	–	–	30,000	83,5	13	64,25	25,7	SE	Ditto.

Table I. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1821.				h.	Inches.	°	Dry.		"		
March	25	2	E	6	30,065	84,5	14,5	64	25,6	SE	Hazy.
	26	1	–	–	30,135	83,4	15	65, 5	26,2	Light SE	Ditto.
	28	2	–	–	30,105	84,0	16	65,25	26,1	SE	Clear.
	29	1	–	–	30,084	84,0	17,5	66	26,4	ENE	Hazy, and clear.
	30	2	–	–	30,110	83,0	16	65, 5	26,2	SE	Clear.
	31	2	–	–	30,065	84,5	15,8	65, 5	26,2	SE	Ditto.
Apr	2	2	–	–	30,125	84,7	16,3	64,75	25,8	SE	Ditto.
	3	2	–	–	30,125	84,0	16	65	26,0	SE	Ditto.
	4	1	–	–	30,128	85,0	20	64, 5	25,8	SE	Ditto.
	5	2	–	–	30,110	84,5	19	64, 5	25,8	SE	Ditto.
	6	2	–	–	30,118	85,0	17	64	25,6	SE	Ditto.
	7	2	–	–	30,110	83,5	17	64, 5	25,8	SE	Ditto.
	8	1	–	–	30,165	84,5	16,5	64	25,6	SE	Ditto.
	9	2	–	–	30,046	84,5	16,5	64, 5	25,8	SE	Ditto.
	10	1	–	–	30,028	85,5	19,5	64, 5	25,8	SE	Hazy,
	11	2	–	–	29,958	87,5	17	63,75	25,4	SE	Clear.
	12	2	–	–	29,955	87,2	18	63,75	25,5	S by E	Ditto.
	13	2	–	–	29,965	86,0	16	64, 5	25,8	S	Ditto.
	14	2	–	–	30,000	85,0	18	64, 5	25,8	SE	Ditto.
	15	2	–	–	30,000	85,0	19	64, 5	25,8	SE	Ditto.
	16	2	–	–	30,000	86,5	19	64,75	25,9	SE	Ditto.
	17	2	–	–	29,974	85,4	16,4	65,75	26,3	SE	Ditto.
	18	2	–	–	29,965	86,0	16	64,25	25,7	SE	Ditto.
	19	2	–	–	29,985	86,0	16	63,75	25,5	SE	Ditto.
	20	2	–	–	29,988	88,2	16	65, 5	26,2	Fresh SE	Ditto.
	21	2	–	–	30,015	86,5	17	64, 0	25,6	SE	Ditto.
	22	2	–	–	30,018	89,8	17,5	64, 5	25,8	SE	Ditto.
	23	2	–	–	29,988	86,8	18	63,75	25,5	SSE	Ditto.
	24	2	–	–	30,008	86,0	18	64,75	25,9	SE	Ditto.
	25	2	–	–	30,015	86,0	16,5	64,75	25,9	SE	Ditto.
	26	1	–	–	30,100	87,5	16	65, 5	26,2	SE	Ditto.
	27	2	–	–	29,965	86,5	16	65,25	26,1	SE	Ditto.
	29	2	–	–	30,000	83,5	18,5	64,25	25,7	SE	Ditto.
	30	2	–	–	30,012	85,6	15,8	65,25	26,1	SE	Ditto.
May	3	1	–	–	30,035	85,5	18	64, 0	25,6	SE	Ditto.
	4	2	–	–	30,026	87,5	16	64, 5	25,8	SE	Ditto.
	5	2	–	–	29,980	87,2	16,2	63, 5	25,4	SSE	Ditto.
	6	1	–	–	29,958	87,5	15,5	64, 5	25,8	SE by S	Ditto.
	7	1	–	–	29,988	87,2	15	65	26,0	SE	Ditto.
	8	2	–	–	29,958	87,0	17,5	64,75	25,9	SE	Ditto.
	9	2	–	–	29,958	87,3	15,5	64,25	25,7	SSE	Ditto.
	10	2	–	–	29,958	87,0	17	64,25	25,7	SE	Somewhat hazy.
	11	2	–	–	29,946	87,4	15,5	64,25	25,7	SE by E	Clear.
	12	2	–	–	29,900	87,0	16	64,75	25,9	SE	Hazy.
	13	2	–	–	29,855	92,5	17	64,75	25,9	SE by E	Clear.
	14	2	–	–	29,858	88,2	15	64,25	25,7	SE	Ditto.
	16	2	–	–	29,945	87,2	19	64, 5	25,8	SE	Ditto.
	17	2	–	–	29922	88,0	16,5	64, 5	25,8	SE	Ditto.

Table I. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1821.				h.	Inches.	°	Dry.		″		
May	18	1	E	6	29,865	87,5	17,5	63, 5	25,4	SE	Clear.
	19	2	–	–	29,778	89,2	21,5	64, 0	25,6	ESE	Ditto.
	20	2	–	–	29,700	89,2	25	64,25	25,7	ESE	Hazy.
	21	1	–	–	29,788	88,5	26	64, 5	25,8	SE	Cloudy.
	22	2	–	–	29,855	88,0	26	64,25	25,7	SSE	Hazy.
	23	1	–	–	29,805	89,2	26	65, 0	26,0	SE	Cloudy.
	24	2	–	–	29,788	89,8	25	64, 0	25,6	SE	Ditto.
	25	2	–	–	29,815	89,5	26	64,75	25,9	SE	Clear.
	26	2	–	–	29,885	88,0	20,5	64,25	25,7	SSE	Ditto.
	27	2	–	–	29,816	89,0	22,5	64, 0	25,6	SE	Ditto.
	28	2	–	–	29,867	86,7	20,0	63	25,2	SE	Ditto.
	29	2	–	–	29,900	89,2	22	62,75	25,1	SE	Ditto.
	30	1	–	–	29,888	87,5	24	63	25,2	SE	Ditto.
	31	2	–	–	29,900	89,2	26	63,25	25,3	S by E	Ditto.
June	1	2	–	–	29,918	87,7	27	64, 5	25,8	SE	Ditto.
	2	1	–	–	29,868	88,3	27	63, 5	25,4	Fresh SE	Hazy.
	4	2	–	–	29,940	90,7	28	63,75	25,5	SE	Clear.
	5	2	–	–	30,000	86,5	20	63,75	25,5	S	Hazy.
	6	1	–	–	29,965	87,2	22	63	25,0	Fresh S by E	Clear.
	8	1	–	–	29,674	86,0	20	64	25,6	SE	Ditto.
	9	2	–	–	29,900	86,4	18	63,75	25,5	SSE	Ditto.
	10	2	–	–	29,905	87,5	22	64, 0	25,6	SE by E	Ditto.
	11	2	–	–	29,858	86,8	19	63,75	25,5	S	Clear; distant lightning.
	12	1	–	–	29,858	87,0	19	65, 0	26,0	SE	Clear.
	13	1	–	–	29,845	88,2	22	65, 0	26,0	Light SE	Ditto.
	14	2	–	–	29,835	83,5	22	63, 5	25,4	SSE	Ditto.
	15	1	–	–	29,865	88,2	22	63	25,2	Fresh SE	Ditto.
	16	1	–	–	29,868	88,5	25	65, 5	26,2	Fresh SE	Ditto.
	17	1	–	–	29,855	88,5	22,0	65, 5	26,2	Fresh SE	Hazy.
	18	1	–	–	29,900	86,6	23,0	66, 0	26,4	Fresh SE	Clear.
	19	2	–	–	29,900	86,0	23,0	63,75	25,5	S	Cloudy.
	20	1	–	–	30,000	86,2	25,5	63	25,2	SE	Clear.
	21	2	–	–	29,945	85,8	25,0	63,75	25,5	Light ESE	Ditto.
	22	2	–	–	29,936	89,0	30,0	63, 5	25,4	SE	Cloudy.
	23	2	–	–	29,900	89,6	32	63,75	25,5	Light SE	Ditto.
	25	1	–	–	29,928	87,2	29,5	64, 5	25,8	SE by E	Ditto.
	26	2	–	–	29,928	86,5	29,5	64,25	25,7	SE	Ditto.
	27	2	–	–	29,945	85,0	23	63,25	25,3	SSW	Ditto.
	28	2	–	–	29,968	85,2	22	63,25	25,3	SW by S	Clear.
		2	M	5	29,988	84,8	27,2	63,25	25,3	NW	Hazy.
	29	2	E	6	29,928	86,2	23	64, 0	25,6	SE	Clear.
		2	M	5	29,975	86,0	25,8	63, 5	25,4	NW	Somewhat hazy.
	30	2	E	6	29,888	91,0	35	62	24,8	SW	Hazy.
		2	M	5	29,925	87,0	34,5	62,75	25,1	W	Clear.
July	1	1	E	6	29,838	90,7	37,5	63	25,2	Light W	Hazy.
		2	M	5	29,874	88,0	35	63,25	25,3	NW	Cloudy.
	3	2	E	6	29,835	91,6	40	63,25	25,3	Fresh W	Ditto.
		2	M	5	29,894	87,0	39,5	64,25	25,7	Light W	Hazy.
	4	1	E	6	29,835	93,5	40	63, 0	25,2	SW	Ditto.

Table I. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1821.					Inches.	°	Dry.		″		
July	4	2	M	5	29,922	89,8	38,5	63,25	25,3	Light NW	Hazy.
	5	2	E	6	29,936	89,5	35,5	63, 5	25,4	W	Clear.
		2	M	5	29,868	88,5	35,0	63, 5	25,4	SE	Flying clouds.
	6	2	E	6	29,878	87,5	32	63, 5	25,4	SSE	Cloudy.
		2	M	5	29,915	88,7	33	63, 5	25,4	WSW	Clear.
	7	2	M	5	29,892	86,4	30	62,75	25,1	SW	Cloudy.
	8	1	E	6	29,908	87,4	31	64	25,6	SE and SW	Ditto.
		2	M	5	29,945	86,7	29,5	63,25	25,3	SW	Ditto.
	9	1	E	6	29,885	88,0	34,5	63, 5	25,4	Light SW	Hazy.
		2	M	5	29,944	86,2	34,7	63,75	25,5	WNW	Ditto.
	10	2	E	6	29,905	88,5	32	62,75	25,1	Light SE	Clear.
		2	M	5	29,934	85,8	31,5	62,75	25,1	W by N	Ditto.
	11	2	E	6	29,835	88,5	33,5	65,25	26,1	SE	Cloudy and rain.
		2	M	5	29,926	86,5	32,6	63	25,2	Light W	Clear.
	12	2	E	6	29,888	86,5	37	63,75	25,5	SE	Ditto.
		2	M	5	29,955	86,5	33	63, 5	25,4	Light W	Hazy.
	13	2	E	6	29,928	86,0	35,5	64, 0	25,6	SE by E	Cloudy.
	14	2	–	–	29,874	85,0	29,5	64,25	25,7	SE by E	Hazy.
		2	M	5	29,873	85,2	29,5	64, 0	25,6	Light SW	Ditto.
	15	2	E	6	29,845	86,5	29,8	64,25	25,7	SE	Ditto.
		2	M	5	29,900	84,5	27	65,25	26,1	Calm	Cloudy.
	16	2	E	6	29,900	84,5	24	64,25	25,7	SSE	Hazy.
		2	M	5	29,915	83,0	23,2	63, 0	25,2		Clear.
	17	2	E	6	29,882	86,0	26,7	64,25	25,7	SE	Ditto.
		2	M	5	29,924	86,7	24,2	63, 5	25,4	Light NW	Ditto.
	18	2	E	6	29,855	88,4	29,5	63,75	25,5	SE	Ditto.
		2	M	5	29,810	84,5	25	63	25,2	SW	Cloudy, and some rain.
	19	2	E	6	29,900	86,2	24,8	63,25	25,3	Calm	Cloudy, and rain at 4 o'clock in the morning.
		2	M	5	29,910	85,0	23,5	64,25	25,7	Calm	Cloudy, and rain in the night.
	20	2	E	6	29,888	86,6	29	64	25,6	SW	Cloudy, and rain.
		2	M	5	29,932	84,5	25,6	63	25,2	SW	Hazy.
	21	1	E	6	29,878	87,2	29,5	63	25,2	W by N	Cloudy.
		2	M	5	29,942	84,2	29,0	62,75	25,1	Light W	Cloudy, and some rain.
	22	2	E	6	29,858	87,5	31	63, 5	25,4	Calm	Hazy.
	23	2	–	–	29,888	88,8	34	62, 5	25,0	SW	Ditto.
		2	M	5	29,944	86,0	32	63, 0	25,2	W	Clear.
	24	2	E	6	29,900	87,2	30	63,75	25,5	SE	Ditto.
		2	M	5	29,945	86,6	30	63,25	25,3	W	Ditto.
	25	2	E	6	29,928	87,5	30	64, 5	25,8	SE	Ditto.
		2	M	5	30,005	86,2	28,5	63, 0	25,2	S by W	Hazy.
	26	2	E	6	29,928	87,2	28,5	63,75	25,5	SE	Ditto.
	27	2	–	–	29,945	86,2	25,5	63, 5	25,4	SSW	Clear.
		2	M	5	29,945	84,2	25,2	64	25,6	Light W	Hazy.
	28	1	E	6	29,905	87,0	31	64	25,6	SW	Clear.
		2	M	5	29,945	86,4	28,7	64,75	25,9	NW	Ditto.
	29	2	M	–	29,960	86,0	25,6	64,75	25,9	Calm	Rain.
	30	2	E	6	29,918	87,0	25,5	63,75	25,5	SE	Clear.
		2	M	5	29,996	85,0	25,2	64, 0	25,6	NW	Hazy; lightning.
	31	2	E	6	29,965	87,8	29,5	64,25	25,7	Light NW	Cloudy.

Table I. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom	Beats.	Seconds.		
1821.				h.	Inches.	″	Dry.		″		
July	31	2	M	5	29,990	85,2	26	63,25	25,3	W	Rain.
Aug.	1	2	E	6	29,948	83,2	22	63,75	25,5	Light SSW	Hazy.
		2	M	5	29,965	83,0	22	63,25	25,3	SW	Cloudy. [ning to the westward.
	2	2	E	6	29,915	84,8	22	64,25	25,7	Calm	Cloudy; distant thunder and light-
		2	M	5	29,925	83,4	22,6	64,25	25,7	SW	Rain.
	3	2	E	6	29,868	87,0	25,5	63,25	25,3	Calm	Hazy.
		2	M	5	29,944	80,0	23,5	64, 0	25,6	Calm	Somewhat hazy.
	4	1	E	6	29,900	85,2	24	64, 0	25,6	W	Cloudy.
		2	M	5	29,922	86,2	23,6	63,25	25,3	Light NW	Cloudy; rain the whole night.
	5	2	E	6	29,900	84,8	24	63, 0	25,2	W	Misty.
		2	M	5	29,985	81,0	23,5	63,25	25,3	SW	Hazy.
	6	2	E	6	29,868	86,0	28,6	62, 5	25,0	SW	Cloudy.
		2	M	5	29,885	82,0	25	62,75	25,1	SW	Hazy.
	7	2	E	6	29,862	86,2	27,5	62,75	25,1	SW	Cloudy and rain.
	8	2	–	–	29,825	85,6	26,5	62,75	25,1	WNW	Ditto, ditto.
		2	M	5	29,860	83,4	27	62, 5	25,0	WNW	Ditto.
	9	2	E	6	29,838	83,5	25,5	63,25	25,3	W	Ditto, and rain.
		2	M	5	29,875	83,6	25,4	64,25	25,7	Light SW	Hazy.
	10	2	E	6	29,878	88,0	32,0	62, 5	25,0	SW	Cloudy.
		2	M	5	29,945	85,4	30.0	63, 0	25,2	SW	Hazy.
	11	2	E	6	29,878	90,4	35,0	63, 0	25,2	SW	Clear.
		2	M	5	29,965	86,4	31,8	64,25	25,7	NW	Ditto.
	12	2	E	6	29,925	90,5	35	63, 5	25,4	SW by N	Hazy.
		2	M	5	29,926	86,0	32,5	63, 5	25,4	W	Somewhat hazy.
	13	2	E	6	29,920	90,5	33,0	63,75	25,5	Calm	Cloudy.
		2	M	5	29,955	86,5	28,8	62,75	25,1	SW	Ditto.
	14	2	E	6	29,965	87,0	28,5	64, 0	25,6	SE	Ditto, lightning.
	15	2	–	–	29,938	88,0	29,5	63,75	25,5	SE	Ditto and some rain.
		2	M	5	29,987	84,5	28	64,25	25,7	NW	Clear.
	16	2	E	6	29,908	87,0	28	64, 0	25,6	SE	Ditto.
		2	M	5	29,940	86,4	28	63,25	25,3	Light SW	Ditto.
	17	2	E	6	29,848	89,8	34	63	25,2	Calm	Ditto.
		2	M	5	29,896	86,0	31,7	63,25	25,3	SW	Ditto.
	18	2	E	6	29,835	87,0	33	64, 5	25,8	Calm	Ditto.
		2	M	5	29,875	85,5	28,4	62, 5	25,0	W	Ditto.
	19	2	E	6	29,835	89,5	33,5	63,25	25,3	Calm	Hazy.
		2	M	5	29,915	87,0	32	64	25,6	Light W	Cloudy.
	20	2	E	6	29,878	90,6	34,5	62,75	25,1	SW	Ditto.
		2	M	5	29,915	86,5	32,5	63, 0	25,2	Light W	Hazy.
	21	2	M	6	29,966	86 0	30	63,75	25,5	NW	Ditto.
	22	2	E	5	29,918	90,5	35	63,25	25,3	Calm	Ditto.
		2	M	6	29,974	86,0	31,5	63,75	25,5	W	Somewhat hazy.
	23	2	E	5	29,935	92,7	35	63,75	25,5	W	Clear.
		2	M	5	29,962	86,6	31,5	64	25,6	NW	Ditto.
	24	2	E	6	29,925	87,5	30	64, 5	25,8	SE	Ditto.
		2	M	5	29,970	86,0	28	63, 0	25,2	SW	Very clear.
	25	1	E	6	29,932	86,7	31,5	63, 5	25,4	S	Clear.
	26	2	–	–	29,935	85,6	28	64, 5	25,8	SSE	Cloudy.

Table I. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1821.				h.	Inches.	°	Dry.		″		
Aug.		2	M	5	29,965	84,0	28	63,75	25,5	W	Hazy.
	27	2	E	6	29,915	87,0	30	64,75	25,9	SE	Very clear.
		2	M	5	29,965	85,0	29,5	63, 5	25,4	Light SW	Cloudy.
	28	2	E	6	29,915	86,6	29,0	64, 5	25,8	SE	Clear.
		2	M	5	29,968	84,5	26,8	63, 5	25,4	SW	Ditto.
	29	2	E	6	29,905	87,5	29,5	64, 5	25,8	SE	Ditto.
	30	1	–	–	29,948	86,5	27	65	26,0	SE	Ditto.
		2	M	5	30,016	85,0	25	63	25,2	S	Cloudy, and lightning.
	31	2	E	6	29,955	85,5	24	65,25	26,1	SE	Clear.
Sep.	1	2	–	–	29,944	85,0	23,5	65	26,0	SE	Ditto.
	2	2	–	–	29,990	86,0	24	65, 5	26,2	E	Cloudy, and thunder.
		2	M	5	30,020	81,2	23,4	63,25	25,3	Fresh SW	Cloudy, and rain in the night.
	3	2	E	6	29,928	87,0	22,0	63,75	25,5	S	Clear.
		2	M	5	9,935	82,4	22	63, 0	25,2	SSW	Cloudy, and some rain.
	4	2	E	6	29,925	86,0	22,5	64,75	25,9	SE	Cloudy.
		2	M	5	29,965	82,7	21,5	65, 0	26,0	Calm	Thin haze.
	5	2	E	6	29,945	87,2	25	64,75	25,0	Light SE	Clear.
		2	M	5	29,986	83,0	21,5	63,25	25,3	Light SW	Clear; and lightning in the NE.
	6	2	E	6	29,938	86,2	23	64,75	25,9	SE	Hazy.
	7	2	–	–	29,945	85,4	23	63,75	25,5	SW	Cloudy.
		2	M	5	29,975	84,4	23	63,25	25,3	SW	Hazy.
	8	2	E	6	29,920	85,4	22	63,75	25,5	SE	Ditto.
		2	M	5	29,965	83,0	21,2	63,25	25,3	SW	Rain in the night.
	9	2	M	5	29,932	82,0	22	65, 5	26,2	NNW. cold	Cloudy; and rain from sun set to sun rise.
	10	2	E	6	29,888	86,0	21	64,25	25,7	Calm	Clear.
		2	M	5	29,915	82,6	19,8	64,75	25,9	Light NW	Rain; lightning, and uncommonly loud thunder.
	11	2	E	6	29,868	86,0	20,0	64, 5	25,8	Calm	Clear.
		2	M	5	29,915	80,5	17,4	63	25,2	SW. cold	Cloudy the whole night.
	12	1	E	6	29,845	84,4	18,2	65, 5	26,2	Calm	Hazy.
		2	M	5	29,875	82,0	16,7	62,25	24,9	Light SW	Rain, with lightning.
	13	2	E	6	29,838	83,0	22	65,75	26,3	Calm	Hazy.
		2	M	5	29,872	80,5	16	64,75	25,9	Ditto	Cloudy; rain at night.
	15	2	E	6	29,865	84,2	17	64	25,6	Ditto	Clear.
		2	M	5	29,925	82,4	15	64,75	25,9	Ditto	Ditto.
	16	2	E	6	29,880	84,5	15	64	25,6	Ditto	Hazy.
		2	M	5	29,925	83,0	15	63,75	25,5	Ditto	Clear.
	17	2	E	6	29,878	86,0	17	65,25	26,1	SE	Hazy.
		2	M	5	29,945	82,4	17,5	64,75	25,9	NW	Clear.
	18	2	E	6	29,920	84,6	19	64,75	25,9	SE	Thin haze.
		2	M	6	29,948	84,0	16,5	64,25	25,7	Calm	Hazy.
	19	2	E	5	29,920	85,6	17,5	64, 5	25,8	Ditto	Clear.
	20	2	–	–	29,888	84,8	17	65	26,0	SE	Hazy.
		2	M	5	29,900	82,4	17	64,25	25,7	NW	Tolerably clear.
	21	1	E	6	29,835	85,5	19	66	26,4	SW	Cloudy.
		2	M	5	29,882	82,6	17,6	64,75	25,9	Calm	Cloudy, with lightning.
	22	2	E	6	29,945	85,0	19	64,25	25,7	Ditto	
	23	2	–	–	29,858	86,0	23	63,75	25,5	SW	Hazy.
		2	M	5	29,884	83,0	27,5	63,25	25,3	W	Mostly cloudy.

Table I. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1821.					Inches.	°	Dry.		"		
Sep.	24	2	M	5	29,916	82,6	23,5	63,75	25,5	Calm	Clear.
	25	2	E	6	29,878	87,0	28	64,75	25,9	Calm	Cloudy.
	26	2	–	–	29,905	86,2	27	63,75	25,5	SW	Hazy.
		2	M	5	29,945	84,5	29,5	63,25	25,3	SW	Cloudy.
	27	2	E	6	29,955	85,6	22	64,75	25,9	SE	Hazy.
		2	M	5	30,018	82,7	18	63,75	29,5	Calm & dew	Clear.
	28	2	E	6	29,955	84,4	20	65,75	26,3	Calm	Cloudy; rain and lightning.
		2	M	5	30,025	81,6	20	64,25	25,7	Light SW	Cloudy, and lightning.
	29	2	E	6	29,970	83,0	20	64, 5	25,8	SW	Cloudy, and rain.
		2	M	5	30,034	80,0	18,8	64, 0	25,6	Calm	Clear.
	30	2	E	6	30,015	84,2	18,5	64,75	25,9	Ditto	Ditto.
		2	M	5	30,074	81,9	18	63,75	25,5	Ditto	Tolerably clear.
Oct.	1	2	E	6	30,038	85,6	20	65	26,0	SE	Clear.
		2	M	5	36,082	82,6	17,8	64,25	25,7	Calm	Mostly clear.
	2	2	E	6	30,005	84,5	19	63,75	25,5	Light SE	Clear.
	3	2	–	–	29,945	85,5	18,5	64,25	25,7	SE	Ditto.
	4	2	–	–	29,900	86,2	17,5	64,75	25,9	Ditto	Ditto.
		2	M	5	29,975	82,5	16	64, 5	25,8	Calm	Clear, and distant lightning.
	5	2	E	6	29,923	85,5	18	64,25	25,7	Ditto	Cloudy.
		2	M	5	29,978	79,5	16,8	64, 5	25,8	SW	Cloudy, and rain.
	6	1	E	6	29,975	85,5	17,5	65, 5	26,2	NE	Cloudy; rain, and lightning with thunder.
		2	M	5	29,985	82,0	17,2	63,25	25,3	W	Hazy, and some rain.
	7	2	E	6	29,965	85,0	16,5	65,25	26,1	NW	Cloudy.
		2	M	5	29,990	81,4	16	65,25	26,1	Calm	Cloudy at sun rise.
	8	1	E	6	29,970	85,0	20	65, 5	26,2	NE	Hazy.
		1	M	5	30,015	83,6	17,2	64, 5	25,8	Calm	Clear.
	9	2	E	6	29,978	86,0	21	66, 5	26,6	Light NE	Ditto.
		2	M	5	29,984	83,0	17,8	65,25	26,1	Calm	Ditto.
	10	2	E	6	29,978	85,0	25	65, 5	26,2	Ditto	Ditto.
		2	M	5	30,030	83,0	19	64, 5	25,8	Ditto	Ditto.
	11	2	E	6	30,005	86,0	27	66, 5	26,6	Ditto	Ditto.
		2	M	5	30,065	82,0	22	64,75	25,9	Ditto	Very clear.
	12	2	E	6	30,018	86,0	29	67	26,8	E	Ditto.
		2	M	5	30,070	79,6	21,5	65,25	26,1	Calm	Ditto.
	13	1	E	6	30,025	86,0	25	66, 2	25,4	Ditto	Ditto.
		2	M	5	30,080	81,0	21	65, 0	26,0	Ditto	Ditto.
	15	2	E	6	30,065	86,7	24	66,25	26,5	E	Ditto.
	16	2	–	–	30,065	87,5	23	66,75	26,7	Ditto	Thin haze.
	17	2	–	–	30,100	83,5	18	67	26,8	NE	Hazy.
	18	2	–	–	30,078	83,0	17	67, 5	27,0	NNE	Cloudy; rain at five o'clock in the afternoon.
	19	1	–	–	30,100	83,0	22	67, 5	27,0	Light NE	Clear.
	20	2	–	–	30,043	83,5	27,5	67, 5	27,0	NE	Ditto.
	21	1	–	–	30,055	82,5	30	67, 5	27,0	Light NE	Ditto.
	22	2	–	–	30,100	79,0	27	67, 0	26,8	N	Ditto.
	23	1	–	–	30,070	84,0	25	68, 5	27,4	NE	Cloudy
	24	2	–	–	30,025	80,0	10	68,25	27,3	Light NE	Clear.
	25	2	–	–	30,044	84,4	12	65,75	26,3	SE	Ditto.
	26	2	–	–	30,060	81,2	13	67,75	27,1	E	Ditto.

Table I. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1821.				h.	Inches.	°	Dry.		″		
Oct.	27	2	E	6	30,055	85 ,0	15,5	67, 0	26, 8	Light NE	Clear.
Nov.	2	2	–	–	30,035	81 ,5	1,5	65, 5	26, 2	Ditto.	Clear.
	3	1	–	–	30,072	77 ,0	0,2	66, 5	26, 6	Calm	Misty.
	4	1	–	–	30,113	83 ,2	3,0	68	27, 2	NE	Clear.
	5	1	–	–	30,126	82 ,3	8	67, 5	27, 0	NE	Hazy.
	6	1	–	–	30,078	82 ,0	10,5	68	27, 2	NE	Hazy.
	7	1	–	–	30,075	82 ,0	13,0	67, 5	27, 0	NE	Clear.
	8	2	–	–	30,000	81 ,5	11	67,25	26, 9	NE. cold	Cloudy, and rain.
	9	2	–	–	30,100	81 ,0	9,5	68,25	27, 3	NE	Clear.
	10	2	–	–	30,055	81 ,6	7	68,75	27, 5	NE	Cloudy, and rain.
Mean					29,992	84,11	19,0		25,869		

TABLE II.

Experiments selected from Table I., *the air having been calm.*

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of Barom.	Height of Therm.	Height of Hygrom.	Number of Beats.	Number of Seconds.	Weather.
1820.				h.	Inches.	°	Dry.		″	
Aug.	25	2	E	6	29,955	87,5	19	63, 5	25, 4	Hazy.
Sept.	16	2	–	6	30,045	83,5	14	64	25, 6	Cloudy.
	21	2	–	6	29,950	82,5	13	64, 5	25, 8	Cloudy.
	23	2	–	6	30,020	87,0	23	64, 5	25, 8	Hazy.
	24	2	–	6	30,100	88,5	27	64, 0	25, 6	Ditto.
	30	2	–	6	30,173	89,2	22	64,25	25, 7	Clear.
Oct.	1	2	–	6	30,200	89,2	27	64, 4	25, 8	Clear.
	2	2	–	6	30,238	91	34	64,85	25, 9	Hazy.
Nov.	8	2	M	5	30,188	76	12	65	26, 0	Clear.
1821.										
Jan.	12	2	E	6	30,220	82	9	67, 5	27, 0	Clear.
	16	2	–	6	30,155	80	8,5	67, 0	26, 8	Hazy.
		1	M	5	30,155	76,2	6	66, 5	26, 6	Clear.
	24	2	E	6	30,055	80,0	12	66,75	26,70	Clear.
	29	2	–	6	30,048	81,0	9,5	66,25	26, 5	Clear.
Feb.	7	2	–	6	30,144	80,5	14	66, 0	26, 4	Clear.
	11	2	–	6	30,215	77,0	16	65, 5	26, 2	Clear.
July	15	2	M	5	29,900	84,5	27	65,25	26, 1	Cloudy.
	19	2	E	6	29,900	86,2	24,8	63,25	25, 3	Cloudy; some rain at 4 o'clock in [the afternoon.
		2	M	5	29,910	85,0	23,5	64,24	25, 7	Cloudy; rain in the night.
	22	2	E	6	29,858	87,5	31	63, 5	25, 4	Hazy.
	29	2	M	5	29,960	86,0	25,6	64,75	25, 9	Rain.
Aug.	2	2	E	6	29,915	84,8	22,0	64,25	25, 7	Cloudy.
	3	2	–	6	29,868	87,0	25,5	63,25	25, 3	Hazy.
		2	M	5	29,944	80,0	23,5	64, 0	25, 6	Hazy.
	13	2	E	6	29,920	90,5	33,0	63,75	25, 5	Cloudy.
	17	2	–	6	29,848	89,8	34,0	63, 0	25, 2	Clear.
	18	2	–	6	29,835	87,0	33	64, 5	25, 8	Clear.

Month.	Day.	Morning or Evening.	Time.	No. of Observations.	Height of Barom.	Height of Therm.	Height of Hygrom.	Number of Beats.	Number of Seconds.	Weather.
1820.			h.		Inches.	°	Dry.		″	
Aug.	19	E	6	2	29,835	89,5	33,5	63,25	25, 3	Hazy.
	22	E	6	2	29,918	90,5	35,0	63,25	25, 3	Ditto.
Sept.	4	M	5	2	29,965	82,7	21,5	65, 0	26, 0	Ditto.
	10	E	6	2	29,888	86,0	21,0	64,25	25, 7	Clear.
	11	E	6	2	29,868	86,0	20,0	64, 5	25, 8	Clear.
	13	E	6	2	29,838	83,0	22,0	65,75	26, 3	Hazy.

Table II. continued.

Time.	Day.	Morning or Evening.	Time.	No. of Observations.	Height of			Number of		Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.	
1822.			h.		Inches.	°	Dry.		″	
Sept,	13	M	5	2	29,872	80, 5	16	64,75	25, 9	Cloudy; rain at night.
	15	E	6	2	29,865	84, 2	17	64, 0	25, 6	Clear.
		M	5	2	29,925	82, 4	15	64,75	25, 9	Clear.
	16	E	6	2	29,880	84, 5	15	64, 0	25, 6	Hazy.
		M	5	2	29,925	83, 0	15	63,75	25, 5	Clear.
	18	–	5	2	29,948	84, 0	16, 5	64,25	25, 7	Hazy.
	19	E	6	2	29,920	85, 6	17, 5	64, 5	25, 8	Clear.
	21	M	5	2	29,818	82, 6	17, 6	64,75	25, 9	Cloudy; with lightning
	22	E	6	2	29,945	85, 0	19, 0	64,25	25, 7	Hazy.
	24	M	5	2	29,916	82, 6	23, 5	63,75	25, 5	Clear.
	25	E	6	2	29,878	87, 0	28, 0	64,75	25, 9	Cloudy.
	27	M	5	2	30,018	82, 7	18, 0	63,75	25, 5	Clear.
	28	E	6	2	29,955	84, 4	20	65,75	26, 3	Cloudy; rain and lightning.
	29	M	5	2	30,034	80, 8	18, 8	64, 0	25, 6	Clear.
	30	E	6	2	30,015	84, 2	18, 5	64,75	25, 9	Clear.
		M	5	2	30,074	81, 0	18, 0	63,75	25, 5	Very clear.
Oct.	1	M	5	2	30,082	82, 6	17, 8	64,25	25, 7	Clear.
	4	M	5	2	29,975	82, 5	16	64, 5	25, 8	Clear, and distant lightning.
	5	E	6	2	29,923	85, 5	18	64,25	25, 7	Cloudy.
	7	M	5	2	29,990	81, 4	16	65,25	26, 1	Cloudy at sun rise.
	8	M	5	1	30,015	83, 6	17, 2	64, 5	25, 8	Clear.
	9	M	5	2	29,984	83, 0	17, 8	65,25	26, 1	Clear.
	10	E	6	2	29,978	85, 0	25, 0	65, 5	26, 2	Clear.
		M	5	2	30,030	83, 0	19	64, 5	25, 8	Clear.
	11	E	6	2	30,005	86, 0	27	66, 5	26, 6	Clear.
		M	5	2	30,065	82, 0	22	64,75	25, 9	Very clear.
	12	M	5	2	30,070	79, 6	21, 5	65,25	26, 1	Clear.
	13	E	6	1	30,025	86, 0	25	66, 0	26, 4	Clear.
	13	M	5	2	30,080	81, 0	21	65, 0	26, 0	Clear.
Nov.	3	E	6	1	30,072	77, 0	0, 2	66, 5	26, 6	Misty.
		Mean	-	-	29,990	83,95	20,31		25,712	

TABLE III.

The wind having been south easterly. Mean of the observations of three days.

Month.	No. of Observations.	Mean height of			Number of		Weather.
		Therm.	Barom.	Hygrom.	Beats.	Seconds.	
		°	Inches.	°		''	
Aug.	6	85,5	29,915	17, 3	63,35	25, 34	Hazy.
Sept.	6	85,6	29,957	17	63,85	25, 54	Hazy.
	6	85,0	29,991	17	64,15	25, 66	Hazy.
	6	82,2	29,960	12	63,35	25, 34	Mostly clear.
	6	34,2	30,005	14	64	25, 6	Mostly cloudy.
	6	85,8	30,055	18, 3	64,25	25, 7	Rather hazy.
Nov.	6	81,6	30,137	10, 7	65, 5	26, 2	Clear generally.
	6	81,2	30,179	6, 2	65, 5	26, 2	Clear.
Feb.	6	80,2	30,120	14	65,65	26, 26	Mostly clear.
	6	78,0	30,118	15	65, 9	26, 36	Ditto.
	6	82,1	30,050	14, 3	65,75	26, 3	Ditto.
March	6	81,8	30,113	17, 6	65, 3	26, 12	Alternately clear and cloudy.
	6	81,9	30,103	17, 5	65, 5	26, 2	Clear.
	6	83,0	29,969	12, 7	64, 4	25, 76	Mostly clear.
	6	83,8	30,092	15, 5	64,97	26, 0	Clear.
April	6	84,4	30,108	16, 0	65, 1	26, 0	Ditto.
	6	84,3	30,113	17, 7	64,35	25, 7	Ditto.
	6	86,4	30,019	17, 2	64, 0	25, 6	Ditto.
	6	85,3	29,985	17, 7	64, 5	25, 8	Ditto.
	6	86,3	29,979	17, 1	64,95	25, 98	Ditto.
	6	86,9	29,996	16, 3	64, 4	25, 76	Ditto.
	6	87,5	30,005	17, 8	64,35	25, 74	Ditto.
	6	85,3	29,990	17, 0	64,75	25, 9	Ditto.
May	6	86,8	30,006	16, 0	64, 4	25, 76	Ditto.
	6	87,0	29,958	17, 3	64,45	25, 78	Somewhat hazy.
	6	89,0	29,900	16, 2	64, 6	25, 82	Clear
	6	87,8	29,908	16, 8	64,45	25, 78	Clear.
	6	88,8	29,777	24, 2	64, 2	25, 68	Mostly hazy.
	6	89,1	29,829	23, 8	64, 3	25, 72	Clear.
	6	88,3	29,876	21, 5	63,25	25, 30	Clear.
	6	89,5	29,916	27	63,85	25, 54	Clear.
	6	86,8	29,902	20	63,85	25, 54	Clear.
	6	85,4	29,864	21, 3	63,65	25, 46	Clear; distant lightning.
	6	88,0	29,927	29	63, 7	25, 48	Cloudy.
	6	85,6	29,947	24, 8	63, 6	25, 40	Cloudy.
July	6	87,4	29,891	30, 0	63,65	25, 46	Mostly cloudy.
	6	87,8	29,876	34, 2	63, 9	25, 60	Clear; cloudy and rain.
	6	85,8	29,882	31, 6	64,15	25, 66	Generally hazy.
	6	86,3	29,879	26, 7	64, 1	25, 64	Clear.
	6	87,3	29,918	29, 5	63,85	25, 54	Clear.
Aug.	6	87,3	29,941	27, 8	63,85	25, 54	Clear.
	6	86,7	29,923	28, 7	64,35	25, 74	Clear.
	6	87,0	29,912	29, 5	64, 5	25, 8	Clear.
Sept.	6	85,2	29,972	24	64,25	25, 7	Clear.
	6	86,7	29,933	23, 2	64, 4	25, 76	Clear.
	6	85,8	29,915	20, 7	64,55	25, 82	Rather hazy.
	6	85,0	29,921	19, 0	64,75	25, 90	Hazy.
Oct.	6	85,2	29,927	19, 2	64,35	25, 74	Clear.
Mean		85,5	29,972	19,96		25,754	

TABLE IV.

The wind having been north easterly. Mean of the observations of three days.

Month.	No. of Observations.	Mean height of			Number of		Weather.
		Therm.	Bar^{o}m.	Hygrom.	Beats.	Seconds.	
1820.			Inches.	Dry.		″	
Oct.	6	82	30,094	2	66, 5	26, 6	Mostly hazy.
Nov.	6	83,9	30,131	5,6	66,65	26, 66	Clear and cloudy alternately.
	6	81,9	30,184	12,3	66,75	26, 70	Clear.
	6	81,5	30,110	4,8	66,65	26, 62	Clear.
Dec.	6	80,9	30,106	4,2	67, 5	27, 00	Clear.
1821.							
Jan.	6	78,4	30,102	5,7	67,85	27, 14	Mostly cloudy.
	6	80,6	30,145	2,7	67,25	26, 90	Clear and cloudy alternately.
	6	79,7	30,139	8,2	67,06	26, 82	Clear.
Feb.	6	79,6	30,150	14,8	67,55	27, 02	Clear.
	6	80,3	30,186	13,2	66, 7	26, 68	Mostly clear.
Oct.	6	84,0	30,081	21	66,75	26, 70	Ditto.
	6	85,9	30,076	21,7	66, 7	26, 68	Mostly hazy.
	6	83,2	30,074	22,2	67,45	26, 98	Cloudy, and rain.
Nov.	6	82,7	30,063	9,3	66, 6	26, 62	Ditto, ditto.
	6	81,1	30,062	16,6	67,65	27 06	Clear.
Mean		81,7	30,113	10,9		26,812	

TABLE V.

The wind having been SW. W. and NW. Mean of the observations of three days.

Month.	No. of Observations.	Mean height of			Number of		Weather.
		Therm.	Barom.	Hygrom.	Beats.	Seconds.	
		°	Inches.	Dry.		″	
Aug.	6	82,9	29,940	11	63, 0	25, 2	Cloudy, and thunder.
Sept.	6	82,2	29,961	19,3	63,35	25, 34	Cloudy, and lightning.
	6	86,7	29,988	19,3	63,92	25, 56	Ditto, ditto.
Dec.	6	85,3	29,973	11,8	63,95	25, 58	Variable.
1821. June	6	87,3	29,950	29,3	62, 9	25, 16	Hazy.
July	6	85,5	29,878	36,5	63, 0	25, 20	Mostly cloudy.
	6	88,3	29,917	37,8	63, 7	25, 48	Ditto hazy.
	6	87,3	29,917	30,8	63,15	25, 12	Hazy.
	6	86,2	29,934	32,9	63,20	25, 28	Generally cloudy.
	6	85,2	29,914	28,6	63, 5	25, 4	Clear.
	6	85,9	29,874	26,0	63, 5	25, 4	Hazy.
	6	85,8	29,921	29,5	62,75	25, 1	Ditto.
	6	86,3	29,964	30,0	63, 0	25, 2	Variable.
	6	85,6	29,945	26,5	64, 1	25, 6	Clear.
	6	86,0	29,984	26,9	63,85	25, 54	Hazy; cloudy and lightning.
Aug.	6	83,2	29,981	22,2	63,75	25, 50	Cloudy; distant thunder and lightning with rain.
	6	82,8	29,936	23,7	63,15	25, 26	Cloudy, and rain.
	6	84,7	29,872	27,0	62,65	25, 06	Cloudy, and rain.
	6	84,2	29,841	26,3	62, 8	25, 12	Rain.
	6	85,7	29,899	29,1	63,25	25, 30	Hazy.
	6	89,1	29,923	33,9	63, 6	25, 4	Somewhat hazy,
	6	85,7	29,956	29,8	63, 5	25, 4	Clear.
	6	86,0	29,904	29,4	63, 0	25, 20	Clear, and somewhat hazy.
	6	88,0	29,903	33,0	63,25	25, 30	Clear, and hazy.
	6	88,2	29,958	32,2	63,75	25, 50	Hazy.
	6	85,0	29,966	28,5	63, 4	25, 36	Clear.
Sept.	6	82,7	29,987	24,9	63, 7	25, 48	Rain, and lightning.
	6	83,6	29,955	22,2	63, 3	25, 32	Cloudy, and some rain; thunder and lightning.
	6	82,5	29,937	21,0	64,45	25, 78	Rain from sun set to sun rise.
	6	81,6	29,912	17,2	63, 3	25, 32	Mostly hazy.
	6	84,9	29,888	22,3	63, 9	25, 56	Hazy.
	6	83,0	29,980	23,2	64, 0	25, 60	Mostly cloudy and rain.
Oct.	6	82,2	29,976	16,8	64,35	25, 74	Ditto, ditto.
Mean		85,1	29,934	26		25,374	

TABLE VI.

Experiments for ascertaining the Motion of Sound with a gun placed on the Ramparts of Fort St. George. Station at the top of the Madras Observatory House.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1820.				h.	Inches.	°	Dry.		″		
July	16	1	E	8	29,875	82,4	16	31,0	12,4	SE	Cloudy.
	17	1	–	8	29,830	83,2	14	31	12,4	SE	Hazy.
	18	1	M	5	29,845	79,8	10	33	13,2	NW	Cloudy.
		1	E	8	29,848	83,9	10	31	12,4	Light SE	Clear.
	19	1	M	5	29,870	79,8	12	32	12,8	Light NE	Cloudy, and rain.
		1	E	8	29,875	82,5	13	30	12,0	SE	Clear.
	20	1	E	8	29,878	83,0	14	30	12,0	SE	Ditto.
	21	1	E	8	29,900	82,0	14	31	12,4	SE	Cloudy.
	22	1	M	5	29,920	80,0	12	31	12,4	W	Ditto.
		1	E	8	29,925	80,4	15	30	12,0	Calm	Ditto.
	23	1	M	5	29,920	80,5	14	31	12,4	Land or W	Ditto.
		1	E	8	29,926	82,0	15	31	12,4	Calm	Ditto.
	24	1	M	5	29,920	80,0	14	31	12,4	W	Hazy.
		1	E	8	30,000	82,8	15	30	12,0	SE	Rain.
	25	1	M	5	29,955	80,2	14	32	12,8	Land	Cloudy.
		1	E	8	29,988	84,0	19	31	12,4	SE	Ditto.
	26	1	M	5	30,045	80,0	16	31	12,4	Light land	Hazy.
		1	E	8	30,040	85,0	20	31	12,4	W	Cloudy.
	27	1	M	5	30,050	82,2	20	30	12,0	W	Hazy.
		1	E	8	30,055	83,0	19	30	12	SE	Thin haze.
	28	1	M	5	30,055	80,7	15	31	12,4	W	Cloudy.
		1	E	8	30,025	81,0	9	30	12,0	Calm	Ditto.
	29	1	M	5	30,020	79,8	9	32	12,8	SE	Variable.
		1	E	8	30,022	79,8	7	31	12,4	SE	Cloudy.
	30	1	M	5	30,040	77,8	7	30	12,0	W	Clear.
		1	E	8	30,008	83,0	7	29	11,6	Fresh SE	Very clear.
	31	1	M	5	30,025	81,0	8	30	12,0	Light SW	Clear.
		1	E	8	30,000	81,7	10	30	12,0	SSE	Cloudy.
Aug.	1	1	E	8	30,015	82,0	7	30	12,0	Light SE	Clear.
	2	1	E	8	30,000	81,0	5	30	12,0	Calm	Cloudy.
	3	1	M	5	30,020	80,2	4	30	12,0	W	Ditto.
		1	E	8	30,025	82,5	6	29	11,6	SE	Clear.
	4	1	–	8	30,015	81,5	8	30	12,0	SE	Ditto.
	5	1	M	5	30,030	80,0	7	29	11,6	Calm	Cloudy.
		1	E	8	29,975	82,0	7	29	11,6	SE	Clear.
	6	1	–	8	29,965	82,5	6	31	12,4	SW	Cloudy.
	7	1	M	5	30,000	80,5	9	30	12,0	Land	Ditto.
		1	E	8	30,015	81,0	8	30	12,0	Calm	Ditto.
	8	2	M	5	30,015	80,2	9	31,5	12,6	W	Ditto.
		2	E	8	29,945	81,0	10	31	12,4	W	Cloudy, and lightning.
	9	1	M	5	29,955	79,8	9	32	12,8	W	Cloudy.
		2	E	8	29,955	82,0	10	32	12,8	Fresh W	Cloudy, and thunder.
	11	2	M	5	30,000	80,6	8	30,5	12,2	Light SW	Hazy.
		2	E	8	29,965	84,0	9	30,0	12,0	SE	Ditto, and lightning.
	12	2	M	5	29,978	81,2	5	31,0	12,4	W	Clear, and distant lightning.
		2	E	8	29,945	83,3	8	30,0	12,0	SE	Hazy, and distant lightning.
	13	2	M	5	30,000	81,4	7	30,5	12,2	Calm	Cloudy, and ditto.

Table VI. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1820.				h	Inches.	°	Dry.		"		
Aug.	13	2	E	8	29,955	84,4	11	31,0	12,4	Light SW	Hazy.
	14	2	M	5	29,970	81,5	11	31,0	12,4	Ditto	Cloudy, and lightning.
		2	E	8	29,950	84,0	11	29,5	11,8	Calm	Ditto.
	15	2	E	8	29,920	83	9	31,0	12,4	W	Hazy.
	16	2	E	8	29,865	84,5	17	30,0	12,0	SE	Cloudy.
	17	2	E	8	29,855	85,0	20	31,5	12,6	Land	Ditto.
	18	2	E	8	29,845	86,0	19	31	12,4	SW	Ditto.
	19	2	–	8	29,800	87,2	21	31	12,4	SW	Ditto.
	20	2	–	8	29,900	85,0	14	30,5	12,2	SE	Clear.
	21	2	M	5	29,960	83,0	14	31,0	12,4	W	Ditto.
		2	E	8	29,915	84,4	14	31,0	12,4	Fresh SE	Ditto.
	22	1	–	8	29,910	82,5	13	30,0	12,0	NW	Cloudy, and thunder to the eastward.
	24	1	M	5	29,998	83,0	15	30,0	12,0	Land	Cloudy.
	25	2	E	8	29,966	85,5	17	30,5	12,2	SE	Ditto.
	26	2	–	8	30,005	84,6	17	30,5	12,2	SE	Clear.
	27	2	–	8	29,932	86,2	19	30,5	12,2	SE	Ditto.
	28	1	M	5	29,965	82,2	20	31,0	12,4	W	Ditto.
		2	E	8	29,910	84,5	24	30,5	12,2	Light SE	Ditto.
	29	1	M	5	29,944	82,8	19	31	12,4	Land	Cloudy, and distant lightning.
		2	E	8	29,935	85,2	19	29,5	11,8	SE	Clear.
	30	2	E	–	29,965	84,5	19	29,5	11,8	SE	Ditto.
	31	2	E	–	29,935	83,5	17	31	12,4	ESE	Ditto.
Sept.	1	2	E	–	29,925	84,0	19	29,5	11,8	SE by E	Ditto.
	2	2	E	–	29,960	85,2	19	30,5	12,2	SE by E	Cloudy.
	3	1	M	5	29,975	81,5	19	32,0	12,8	WSW	Ditto.
		2	E	8	29,965	82,2	19	30,5	12,2	Light SW	Clear, and distant lightning.
	4	2	E	–	29,940	84,0	17	30,0	12,0	SE by S	Clear, ditto.
	5	1	M	5	29,970	84,0	15	32,0	12,8	W by S	Clear.
		2	E	8	29,965	84,0	15	30,5	12,2	SE by S	Clear, and distant lightning.
	6	2	E	–	29,970	82,6	19	31,0	12,4	Calm	Thin haze.
	7	2	E	–	29,965	84,4	15	30,0	12,0	SE	Haze.
	8	1	M	5	29,976	83,2	17	31,0	12,4	W by S	Hazy, and lightning.
		2	E	8	29,970	84,0	15	30,5	12,2	SE by S	Vivid lightning and thunder.
	9	1	E	–	30,000	83,5	18	30,0	12,0	SE by S	Clear.
	10	1	E	–	29,975	84,2	17	30,0	12,0	SSE	Clear, and vivid lightning and thunder.
	12	2	E	–	29,975	84,5	14	31,0	12,4	Light NE	Ditto, ditto, ditto.
	13	2	E	–	29,930	79,5	12	31,0	12,4	SE	Flying clouds.
	14	2	E	–	29,915	81,5	7	30,5	12,2	SE	Ditto.
	15	2	E	–	29,960	83,2	14	30,5	12,2	SE	Ditto.
	16	2	E	–	29,948	83,0	13	30,5	12,2	Light SE	Ditto.
	17	2	E	–	30,045	81,5	13	30,5	12,2	SE by E	Haze.
	18	1	E	–	30,010	83,0	13	31,0	12,4	Calm	Hazy.
	19	2	E	–	30,020	84,5	15	31,5	12,6	SE by E	Cloudy.
	20	2	E	–	30,020	82,0	15	31,0	12,4	Calm	Ditto.
	21	2	E	–	29,950	82,0	11	31,0	12,4	W by N	Ditto.
	22	2	E	–	29,958	83,5	14	31,0	12,4	SE by E	Hazy.
	23	2	E	–	30,050	86,6	21	30,5	12,2	Calm	Ditto.
	24	2	E	–	30,088	85,5	26	30,5	12,2	Ditto	Clear.
	25	2	E	–	30,045	85,0	20	30,5	12,2	SE	Ditto.
	26	2	E	–	30,000	83,5	18	31,0	12,4	SE by E	Flying clouds.
	27	–	–	–	30,062	85,0	15	30,5	12,2	SE	Clear; some lightning.
	28	1	–	–	30,070	83,2	17	32,0	12,8	SW	Flying clouds; lightning.
	29	1	–	–	30,166	88,2	24	31,0	12,4	NW	Clear; lightning to the south.

Table VI. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1820.				h.	Inches.	°	Dry.		″		
Sept.	30	2	E	8	30,200	87,0	21	31, 3	12,52	Calm	Clear; lightning to SW. quarter.
Oct.	1	–	–	–	30,210	86,5	22	30, 3	12,12	Ditto	Ditto; ditto to the north.
	2	–	–	–	30,230	87,2	26	30, 7	12,28	Ditto	Ditto.
	3	–	–	–	30,200	85,2	27	30, 5	12, 2	Ditto	Ditto.
	4	1	–	–	30,145	85,2	23	30, 5	12, 2	SE	Hazy.
	5	1	–	–	30,040	84,8	19	30, 0	12, 0	Ditto	Clear.
	6	1	–	–	30,050	86,4	18	30, 0	12, 0	SE by E	Flying clouds.
	7	–	–	–	30,088	85,8	20	29, 5	11, 8	SE	Cloudy.
	8	–	–	–	30,100	83,5	21	30, 0	12, 0	Ditto	Clear.
	9	–	–	–	30,075	83,5	19	30, 0	12, 0	Ditto	Ditto.
	10	–	–	–	30,075	83,5	20	30, 5	12, 2	Ditto	Ditto.
	11	–	–	–	30,075	83,2	19	30, 0	12, 0	Ditto	Ditto.
	12	–	–	–	30,038	83,5	21	30	12, 0	Ditto	Ditto; lightning to the north.
	13	–	–	–	30,055	84,5	20,5	30	12	NE by E	Ditto.
	14	–	–	–	30,082	84,0	21	30, 5	12, 2	NE	Ditto.
	15	–	–	–	30,120	85,2	21	31, 5	12, 6	E by S	Ditto.
	16	–	–	–	30,124	84,4	20,5	31, 0	12, 4	Light NE	Rather hazy.
	18	–	–	–	30,038	81,0	15,5	30, 5	12, 2	NE by N	Cloudy.
	19	–	–	–	30,050	77,0	6	31, 5	12, 6	NE	Rain.
							Damp				
	20	–	–	–	30,138	77,0	2	31, 0	12, 4	N	Hazy.
	21	–	–	–	30,080	80,0	3	30, 5	12, 2	NE by N	Clear.
	22	–	–	–	30,065	81,0	1,5	32, 0	12, 8	NNW	Cloudy; thunder and lightning.
	24	–	–	–	30,088	80,0	1,5	31, 0	12, 4	Calm	Clear.
							Dry.				
	25	–	–	–	30,112	80,8	7,5	31, 5	12, 6	Ditto	Ditto.
	26	–	–	–	30,120	78,5	11	32, 0	12, 8	Ditto	Ditto.
	27	2	–	–	30,135	80,2	13,5	31,25	12, 5	NE light	Ditto.
	28	1	–	–	30,135	82,0	11,5	31, 0	12, 4	NE	Cloudy.
	29	2	–	–	30,124	81,5	11,5	30,75	12, 3	NNE	Ditto.
							Damp				
	31	2	–	–	30,085	79,0	4	31	12, 4	NW	Hazy and clear.
Nov.	1	–	–	–	30,118	82,0	4	30, 5	12, 2	Calm	Clear; distant lightning in the SW.
							Dry.				
	3	–	–	–	30,125	82,5	4	30, 7	12,28	Ditto	Ditto.
	4	–	–	–	30,110	81,3	6	31, 5	12, 6	Ditto	Ditto.
	5	–	–	–	30,128	82,0	10	31, 0	12, 4	ENE	Ditto.
	6	–	–	–	30,145	80,4	15	30, 5	12, 2	E	Ditto.
	7	1	M	5	30,170	77,4	11	31, 0	12, 4	S by E light	Ditto.
		2	E	8	30,172	79,0	15	30,75	12, 3	Calm	Ditto.
	8	1	M	5	30,188	76,0	12	30, 0	12, 0	Ditto	Ditto.
		2	E	8	30,186	80,4	16	30,25	12, 1	Ditto	Ditto.
	9	2	M	5	30,188	80,8	12,5	31,25	12, 5	NNW	Ditto.
		–	E	8	30,188	80,4	13	30, 5	12, 2	NNE	Ditto.
	10	–	M	5	30,178	79,6	11	30, 5	12, 2	NE	Ditto.
		2	E	8	30,178	82,0	11,5	30, 5	12, 2	Ditto	Ditto.
	12	2	–	–	30,205	81,5	12,5	30,75	12, 3	E by N	Cloudy.
	13	–	–	–	30,165	78,4	8	30,75	12, 3	NE	Ditto.
	14	–	–	–	30,165	78,4	1,5	30, 5	12, 2	Ditto	Clear.
	15	1	–	–	30,130	79,2	3	30, 0	12, 0	Ditto	Cloudy.
	16	2	–	–	30,130	82,0	6	30, 0	12, 0	Ditto	Clear.
	17	–	–	–	30,170	81,0	15	30, 5	12, 2	ENE	Rather hazy.
	18	–	–	–	30,120	80,0	5	30, 0	12, 0	NE	Cloudy.

Table VI. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1820.				h.	Inches.	°	Dry.		″		
Nov.	19	2	E	8	30,110	79,5	4	30,75	12,3	NE	Clear.
	20	–	–	–	30,120	78,0	10	30,75	12,3	ENE	Cloudy.
	21	–	–	–	30,085	78,5	9,5	30,25	12,1	NNE	Clear.
	22	–	–	–	30,075	80,0	9	30,75	12,3	NE	Ditto.
	23	–	–	–	30,110	78,5	12	30,75	12,3	Ditto	Ditto.
	24	–	–	–	30,145	78,8	13	30,75	12,3	Ditto	Ditto.
	25	–	–	–	30,110	78,5	10,5	31, 0	12,4	NE by N	Ditto.
	27	–	–	–	30,180	79,0	14	31, 0	12,4	ENE	Ditto.
	28	1	–	–	30,188	78,0	15	32	12,8	NE	Ditto.
	29	2	–	–	30,210	78,0	19	31,25	12,5	Ditto	Cloudy.
							Damp				
Dec.	4	–	–	–	30,030	75,2	9	31, 5	12,6	NNW	Clear.
	5	–	–	–	30,100	77,2	4	31, 0	12,4	Calm	Ditto.
	6	–	–	–	30,088	78,5	6,5	30, 5	12,2	ENE	Clear.
	7	–	–	–	30,110	78,0	3	31, 0	12,4	NE	Ditto.
							Dry.				
	8	–	–	–	30,160	77,8	6	30,75	12,3	NNE	Ditto.
	10	–	–	–	30,120	77,4	8	31, 0	12,4	NE	Ditto.
		–	M	5	30,115	76,0	4	31,25	12,5	N	Ditto.
	11	–	E	8	30,115	76,0	12	31,25	12,5	NNE	Ditto.
	12	–	M	5	30,078	75,0	5	31,25	12,5	N b W light	Hazy.
		–	E	8	30,135	78,0	10	30, 5	12,2	NE	Cloudy.
							Damp				
	14	1	M	5	30,074	77,0	1,5	31, 0	12,4	Ditto	Clear.
							Dry.				
		2	E	8	30,100	79,0	2	30,25	12,1	E	Ditto.
	15	2	–	–	30,110	79,5	4,5	30, 5	12,2	Ditto	Ditto.
	16	1	M	5	30,095	78,0	1,0	30	12,0	E by E	Cloudy.
		2	E	8	30,125	79,5	8,5	31, 0	12,4	ENE	Clear.
	17	1	M	5	30,122	77,2	2,	30, 5	12,2	NW light	Ditto.
		2	E	8	30,135	79,2	6,5	30,75	12,3	NE	Ditto.
	18	1	M	5	30,135	75,0	5	31, 0	12,4	NNE	Ditto.
	19	–	–	–	30,120	77,0	5	30, 5	12,2	NW	Clear and dew.
		–	E	8	30,115	78,5	15	30, 0	12,0	E by N	Hazy.
	20	–	–	–	30,045	76,7	7	31, 5	12,6	NE	Cloudy.
							Damp				
	24	–	–	–	30,120	80,0	1	30, 0	12,0	Ditto	Clear.
	25	2	–	–	30,048	78,2	12	31, 0	12,4	Ditto	Ditto.
	26	1	–	–	30,058	84,5	9	30, 5	12,2	E	Ditto.
	27	2	–	–	30,075	79,2	6	31, 0	12,4	ENE	Hazy.
							Dry.				
	28	2	–	–	30,075	78,5	2,5	30,75	12,3	Ditto	Clear.
	29	1	–	–	30,110	79,2	5	31, 0	12,4	NE	Haze.
	30	2	–	–	30,100	79,0	9,5	31,25	12,5	Ditto	Cloudy
1821.							Damp				
Jan.	3	2	–	–	30,028	75,5	4	31, 5	12,6	Ditto	Clear.
	4	1	M	5	30,032	74,0	6	32, 0	12,8	Ditto	Ditto.
		–	E	8	30,065	75,5	0,5	31, 0	12,4	Ditto	Ditto.
	5	–	M	5	30,100	76,0	4	31, 5	12,6	Ditto	Hazy.
		–	E	8	30,128	77,2	3	30, 5	12,2	Ditto	Clear.
	6	2	–	–	30,138	75,5	3,5	31,25	12,5	Ditto	Ditto.
	7	–	–	–	30,150	77,2	7	31,25	12,5	Ditto	Cloudy.
	8	–	–	–	30,166	76,5	7	31	12,4	NE	Clear.

Table VI. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1820.				h.	Inches.	°	Damp		″		
Jan.	9	2	E	8	30,205	77,5	1	31	12,4	Calm	Clear.
	10	1	M	5	30,200	75,2	1,5	31, 5	12,6	Ditto	Ditto.
							Dry.				
		2	E	8	30,188	77,2	3	30,75	12,3	Ditto	Ditto.
	11	1	M	5	30,200	74,6	1	31, 5	12,6	NW light	Ditto.
		2	E	8	30,218	75,8	7	31	12,4	NNE	Ditto.
	13	1	–	–	30,200	77,2	7	31	12,4	ENE	Ditto.
	14	2	–	–	30,200	77,5	8,5	31	12,4	Calm	Hazy.
	15	–	–	–	30,165	78,2	7,5	31,25	12,5	Light NE	Clear.
	16	1	M	5	30,155	76,2	6	30, 5	12,2	Calm	Ditto.
		2	E	8	30,160	77,5	9	30,75	12,3	Ditto	Ditto.
	17	–	–	–	30,175	77,2	10	30,75	12,3	East	Ditto.
	18	1	M	5	30,200	76,5	8	30, 5	12,2	Calm	Ditto, and dew.
	19	2	E	8	30,156	79,2	3	31,75	12,7	Ditto	Cloudy.
	20	–	–	–	30,130	79,0	5	31	12,4	East	Ditto.
	21	1	M	5	30,139	76,5	5,5	31	12,4	NE	Clear.
		2	E	8	30,138	77,5	11,5	31,25	12,5	NE light	Ditto.
	22	2	–	–	30,118	78,0	8,5	31, 0	12,4	Ditto	Ditto.
	23	–	–	–	30,120	78,5	11	30, 5	12,2	NE	Ditto.
	24	2	–	–	30,080	78,5	11,5	31, 0	12,4	Calm	Ditto.
	25	1	–	–	30,100	79,2	10	31, 0	12,4	NE	Hazy.
		1	M	5	30,132	77,0	10	31, 0	12,4	E	Clear, and dew.
	26	2	E	8	30,118	79,0	10,5	31, 0	12,4	NE	Hazy.
	27	1	M	5	30,115	76,5	9	30, 5	12,2	Calm	Flying clouds.
		2	E	8	30,115	78,2	9,5	31, 5	12,6	NE	Hazy.
	28	1	–	–	30,050	80,5	9,5	30, 0	12,0	Ditto	Clear.
	29	1	–	–	30,050	77,8	9	30, 5	12,2	SE light	Ditto.
	30	2	–	–	30,035	77,8	12	31,25	12,5	Calm	Ditto.
	31	–	–	–	30,005	77,8	8,5	30, 5	12,2	SE	Hazy.
Feb.	1	1	M	5	29,998	74,5	8,5	32	12,8	Calm	Ditto.
		2	E	8	30,020	77,5	10	30	12,0	E by S	Clear.
	2	–	–	–	30,028	77,2	12,5	30,75	12,3	NNE	Hazy.
	3	–	–	–	30,105	80,5	13	30,75	12,3	E	Clear.
	4	–	–	–	30,168	78,0	11,5	31,25	12,5	NE	Ditto.
							Damp				
	5	1	M	5	30,110	76,2	11	31, 5	12,6	Light NE	Clear.
		2	E	8	30,125	77,5	13	30, 5	12,2	East	Ditto.
	6	2	–	–	30,125	76,0	12	30, 5	12,2	Calm	Ditto.
		1	M	–	30,100	72,0	12,5	31, 5	12,6	Ditto	Ditto.
	7	2	E	8	30,148	75,5	13,5	31,25	12,5	Ditto	Ditto.
		1	M	5	30,135	73,0	12	30, 5	12,2	Ditto	Ditto.
	8	2	E	8	30,145	74,5	27	30,75	12,3	Ditto	Ditto.
		1	M	5	30,155	72,5	12	31, 0	12,4	NW	Ditto.
	9	2	E	8	30,145	73,5	23	31, 0	12,4	Calm	Ditto.
	10	2	–	–	30,200	76,0	20	31, 0	12,4	Ditto	Ditto.
	11	–	–	–	30,200	80,0	17,5	31, 0	12,4	Light E	Ditto.
	12	–	–	–	30,215	77,0	12,0	30,75	12,3	NE	Ditto.
	13	–	–	–	30,192	78,2	13,5	31,25	12,5	ENE	Ditto.
		1	M	5	30,210	76,0	11	30, 0	12,0	NW	Thin haze.
	14	1	E	8	30,165	78,2	14	30, 5	12,2	NE	Clear.
	15	2	–	–	30,184	78,6	14	31,25	12,5	Calm	Ditto.
	16	–	–	–	30,188	78,0	11,5	31, 0	12,4	Ditto	Ditto.
		1	M	5	30,188	75,5	13,0	31, 0	12,4	Ditto	Ditto.

Table VI. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Wind.	Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.		
1820.				h.	Inches.	″	Damp		″		
Feb.	17	2	E	8	30,182	76, 0	14	31,75	12, 7	Calm	Clear.
	18	–	–	–	30,092	77, 8	15	30,75	12, 3	Ditto	Ditto.
		1	M	5	30,165	73, 5	14, 5	31, 0	12, 4	Ditto	Ditto.
	19	2	E	8	30,085	78, 2	14, 5	31,25	12, 5	SE	Ditto.
	20	–	–	–	30,135	77, 8	13, 5	30,75	12, 3	Light SE	Ditto.
	21	–	–	–	30,144	79, 2	14	30,75	12, 3	SE	Ditto.
	22	–	–	–	30,118	79, 5	14, 5	31, 5	12, 6	SE by E	Ditto.
	23	–	–	–	30,118	79, 5	16, 5	31,25	12, 5	E by S	Ditto.
	24	–	–	–	30,125	80, 4	16, 5	30, 5	12, 2	SE	Ditto.
	25	–	–	–	30,115	80, 5	15, 5	31, 0	12, 4	SE by E	Ditto.
	26	–	–	–	30,120	81, 2	14	31,25	12, 5	SE	Ditto.
	27	1	–	–	30,040	85, 5	14	30, 5	12, 2	Ditto	Ditto.
	28	2	–	–	30,035	81, 2	14	31, 0	12, 4	Ditto	Ditto.
March	1	–	–	–	30,068	81, 5	14, 5	31,25	12, 5	Ditto	Ditto.
	2	–	–	–	30,125	81, 5	15, 5	30, 5	12, 2	Calm	Ditto.
	3	–	–	–	30, 11	79, 2	17, 5	30, 5	12, 2	Ditto	Ditto.
							Dry.				
	4	–	–	–	30,048	79, 2	19	30,75	12, 3	SE	Ditto.
	5	–	–	–	30,078	80, 2	18	30,75	12, 3	Ditto	Ditto.
	6	–	–	–	30,140	79, 5	20	31, 0	12, 4	Ditto	Ditto.
	7	–	–	–	30,125	80, 8	19	30, 5	12, 2	Ditto	Ditto.
		1	M	5	30,125	75, 5	19	31, 0	12, 4	Ditto	Ditto.
	8	2	E	8	30,115	81, 5	18, 5	31, 5	12, 6	Ditto	Ditto.
	9	–	–	–	30,124	81, 0	14	31, 0	12, 4	Calm	Cloudy; lightning.
	10	–	–	–	30,085	80, 5	10	30,75	12, 3	Light SE	Cloudy.
	11	–	–	–	30,025	82, 0	12	31, 0	12, 4	SE	Clear.
	12	–	–	–	30,018	82, 0	12	30, 5	12, 2	SE	Ditto.
	13	–	–	–	30,058	82, 0	12	30,25	12, 1	SE	Ditto.
	14	–	–	–	30,120	83, 0	14	31, 5	12, 6	SSW	Ditto.
		1	M	5	30,100	81, 4	11, 5	31, 0	12, 4	SE	Ditto.
	15	2	E	8	30,110	83, 4	14	30,75	12, 3	SE	Ditto.
		1	M	5	30,125	81, 4	14	31	12, 4	Light SW	Ditto.
	18	2	E	8	29,998	81, 5	12	31, 0	12, 4	SE	Clear, and haze.
	19	–	–	–	29,988	81, 0	11	30,75	12, 3	SE by E	Cloudy.
	20	–	–	–	29,968	83, 5	12	31, 5	12, 6	S	Ditto.
	21	1	–	–	30,015	82, 8	11	31, 0	12, 4	SE	Clear.
	22	–	–	–	29,980	83, 0	11	31, 0	12, 4	Ditto	Ditto.
	23	–	–	–	30,015	83, 0	12, 5	30, 5	12, 2	Ditto	Ditto.
	24	2	–	–	30,015	83, 0	12, 5	30,75	12, 3	Dttto	Ditto.
	25	–	–	–	30,100	83, 5	14	30,75	12, 3	Light ditto	Ditto.
	26	1	–	–	30,138	82, 4	15	30, 5	12, 2	Ditto	Ditto.
	27	–	–	–	30,085	83, 0	18	30, 0	12, 0	Calm	Cloudy.
	28	–	–	–	3008 5	83, 0	18	30, 0	12, 0	SE	Clear.
Mean	-	-	-		30,065	80,47	11,36		12,306		

TABLE VII.

Experiments selected from Table VI. *the air having been calm.*

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.	
1820.				h.	Inches.	°	Dry.		″	
July.	22	1	E	8	29,925	80,4	15	30	12	Cloudy.
	23	–	–	–	29,926	82,0	15	31	12, 4	Ditto.
	28	–	–	–	30,025	81,0	9	30	12, 0	Ditto.
Aug.	2	–	–	–	30,000	81,0	5	30	12, 0	Ditto.
	5	1	M	5	30,030	80,0	7	29	11, 6	Ditto.
	7	–	E	8	30,015	81,0	11	30	12, 0	Ditto.
	13	2	M	5	30,000	81,4	7	30, 5	12, 2	Ditto.
	14	2	E	8	29,950	84,0	11	29, 5	11, 8	Ditto; some rain, with lightning.
Sept.	6	2	–	–	29,970	82,6	19	31, 0	12, 4	Thin haze.
	18	1	–	–	30,010	83,0		31, 0	12, 4	Clear.
	20	2	–	–	30,020	82,0	15	31, 0	12, 4	Cloudy.
	23	2	–	–	30,050	86,6	21	30, 5	12, 2	Thick haze.
	24	2	–	–	30,088	85,5	26	30, 5	12, 2	Clear.
	30	–	–	–	30,200	87,0	21	31, 3	12,56	Clear; lightning SW. quarter.
Oct.	1	–	–	–	20,210	86,5	22	30, 3	12,16	Clear; lightning to the north.
	2	–	–	–	30,230	87,2	26	30, 7	12,28	Clear.
	3	–	–	–	30,200	85,2	27	30, 5	12, 2	Ditto.
	24	1	–	–	30,088	80,0	Damp 1,5	31, 0	12, 4	Ditto.
	25	–	–	–	30,112	80,8	Dry. 7,5	31, 5	12, 6	Ditto.
	26	–	–	–	30,120	78,5	11	32, 0	12, 8	Ditto.
Nov.	1	2	–	–	30,118	82,0	Damp 4	30, 5	12, 2	Clear; distant lightning in the SW.
	3	2	–	–	30,125	82,5	Dry. 4	30,75	12, 3	Clear.
	4	2	–	–	30,110	81,3	6	31, 5	12, 6	Ditto.
	7	2	–	–	30,172	79,0	15	30,75	12, 3	Ditto.
	8	1	M	5	30,188	76,0	12	30	12, 0	Ditto.
		2	E	8	30,186	80,4	16	30,25	12, 1	Ditto.
Dec.	5	2	–	–	30,100	77,2	Damp 4	31, 0	12, 4	Ditto.
1821. Jan.	9	–	–	–	30,205	77,5	1	31, 0	12, 4	Ditto.
	10	–	M	5	30,200	75,2	1,5	31, 5	12, 6	Ditto.
		1	E	8	30,188	77,2	Dry. 3	30,75	12, 3	Ditto.
	14	2	–	–	30,200	77,5	8,5	31	12, 4	Hazy.
	16	2	M	5	30,155	76,2	6	30, 5	12, 2	Clear.
		1	E	8	30,160	77,5	9	30,75	12, 3	Ditto.

Table VII. continued.

Month.	Day.	No. of Observations.	Morning or Evening.	Time.	Height of			Number of		Weather.
					Barom.	Therm.	Hygrom.	Beats.	Seconds.	
1821.				h.	Inches.	°	Dry.		″	
Jan.	18	1	M	5	30,200	76,5	8	30, 5	12, 2	Clear, and dew.
	19	2	E	8	30,156	79,2	3	31,75	12, 7	Cloudy.
	24	2	–	–	30,080	78,5	11, 5	31, 0	12, 4	Clear.
	27	1	M	5	30,115	76,5	9	30, 5	12, 2	Flying clouds.
	28	1	E	8	30,050	80,5	9, 5	30, 0	12, 0	Clear.
	30	2	–	–	30,035	77,8	12	31,25	12, 5	Ditto.
Feb.	1	1	M	5	29,998	74,5	8, 5	32, 0	12, 8	Hazy dew.
	6	2	E	8	30,125	76,0	12	30, 5	12, 2	Clear.
		1	M	5	30,100	72,0	12, 5	31, 5	12, 6	Ditto.
	7	2	E	8	30,148	75,5	13, 5	31,25	12, 5	Ditto.
		1	M	5	30,135	73,0	12, 0	30, 5	12, 25	Ditto.
	8	2	E	8	30,145	74,5	27	30,75	12, 3	Ditto.
	9	–	–	–	30,145	73,5	23	31, 0	12, 4	Ditto.
	10	–	–	–	30,200	76,0	20, 0	31, 0	12, 4	Ditto.
	15	–	–	–	30,184	78,0	14	31,25	12, 5	Ditto.
	16	–	–	–	30,188	78,0	15, 5	31	12, 4	Ditto.
		1	M	5	30,188	75,5	13, 0	31	12, 4	Ditto.
	17	2	E	8	30,182	76,0	14, 0	31,75	12, 7	Ditto.
	18	2	–	–	30,092	77,8	15	30,75	12, 3	Ditto.
		1	M	5	30,165	73,5	14, 5	31, 0	12, 4	Ditto.
March	2	2	E	8	30,125	81,5	15, 5	30, 5	12, 2	Ditto.
	3	–	–	–	30,110	79,2	17, 5	30, 5	12, 2	Ditto.
	9	–	–	–	30,124	81,0	14	31, 0	12, 4	Cloudy and lightning.
	27	1	–	–	30,085	83,0	18	30, 0	12, 2	Cloudy.
Mean	-	-	-		30,111	79,3	11,85		12,313	

TABLE VIII.

The wind having been south-easterly, mean of the observations of three days.

Month.	No. of Observations.	Mean height of			Number of		Weather.
		Barometer.	Therm.	Hygrom.	Beats.	Seconds.	
		Inches.	°	Dry.		″	
	6	29,925	83,9	11, 3	30, 0	12, 0	
	6	29,927	85,0	15	30,65	12, 26	
	6	29,957	85,3	18, 3	30,15	12, 06	
	6	29,942	84,0	18, 3	30, 0	12, 0	
	6	29,955	84,4	17	30,35	12, 14	
	6	29,955	82,6	14	30, 5	12, 2	
	6	29,941	82,6	11, 3	30, 5	12, 2	
	6	30,008	83,2	14	31, 0	12, 4	
	6	30,086	84,5	17, 7	30,65	12, 26	
	6	30,025	77,5	9, 2	30, 3	12, 12	
	6	30,155	78,4	14	30, 9	12, 36	
	6	30,120	79,8	15, 8	31,05	12, 42	
	6	30,090	81,0	14, 3	31, 2	12, 04	
	6	30,065	80,3	17, 2	30, 9	12, 36	
	6	30,127	80,6	19, 2	31, 0	12, 4	
	6	30,043	81,5	11, 3	30,75	12, 3	
	6	30,055	82,3	12, 7	30, 7	12, 28	
	6	29,990	82,5	11, 8	31, 1	12, 44	
	4	30,084	83,5	15	30,37	12, 14	
Mean - -		30,023	82,3	14,60		12,231	

TABLE IX.

The wind having been north-easterly, mean of the observations of three days.

Month.	No. of Observations.	Mean height of			Number of		Weather.
		Barometer.	Therm.	Hygrom.	Beats.	Seconds.	
1820.		Inches.	°	Dry.		″	
Oct.	6	30,132	81,3	12, 0	30,75	12, 3	Clear.
Nov.	6	30,181	80,7	11, 8	30, 5	12, 2	Ditto.
	6	30,178	79,4	7, 3	30, 7	12, 28	Cloudy.
	6	30,140	81,0	8, 7	30,15	12, 06	Ditto.
	6	30,105	78,7	7, 8	30, 6	12, 24	Clear.
	6	30,110	79,1	11, 3	30,75	12, 30	Ditto.
	6	30,170	78,5	14, 5	31, 1	12, 44	Ditto.
Dec.	6	30,119	78,1	1, 2	30,75	12, 3	Ditto.
	6	30,117	76,4	8, 0	31, 2	12, 48	Ditto.
	6	30,112	79,3	5, 0	30, 6	12, 24	Ditto.
	6	30,076	78,0	0, 5	31,05	12, 4	Ditto.
	6	30,083	78,9	2, 0	31, 0	12, 4	Cloudy.
				Damp			
Jan.	6	30,105	76,1	4, 8	31,35	12, 54	Clear.
				Dry.			
	6	30,183	76,8	2, 5	31, 1	12, 44	Ditto.
	6	30,148	77,9	8, 8	31, 0	12, 4	Cloudy.
	6	30,119	78,5	10, 0	30,85	12, 34	Clear.
Feb.	6	30,103	77,8	11, 2	31,15	12, 46	Ditto.
	6	30,180	78,2	14, 2	30,75	12, 3	Ditto.
Mean	-	30,131	78,6	7,33		12,340	

TABLE X.

The wind having been SW by W, and NW, mean of the observations of three days.

Month.	No. of Observations.	Mean height of			Number of		Weather.
		Barometer.	Therm.	Hygrom.	Beats.	Seconds.	
1820.		Inches.	°	Dry.		″	
August.	6	29,972	81, 0	9, 7	31, 5	12,60	Cloudy and lightning.
	6	29,974	82, 0	8, 0	30,85	12,34	Mostly hazy.
	6	29,915	83, 2	13, 3	31,15	12,46	Ditto, ditto.
	6	29,868	85, 4	18, 0	31, 0	12, 4	Cloudy.
Sep. and Nov.	6	30,074	81, 8	16, 2	31, 0	12, 4	Mostly cloudy.
Dec. and Mar.	6	30,076	77, 7	3, 3	31, 4	12,56	Hazy.
Mean		29,979	81,85	11,41		12, 46	

TABLE XI.

Mean motion of sound for each month, according to the experiments with the Mount gun.

Month.	Mean height of			Velocity in a second.
	Barometer.	Therm.	Hygrom.	
	Inches.	°	Dry.	Feet.
January.	30,124	79,05	6, 2	1101
February	30,126	78,84	14,70	1117
March.	30,072	82,30	15,22	1134
April.	30,031	85,79	17,23	1145
May.	29,892	88,11	19,92	1151
June.	29,907	87,10	24,77	1157
July.	29,914	86,65	27,85	1164
August.	29,931	85,02	21,54	1163
September.	29,963	84,49	18,97	1152
October.	30,058	84,33	18,23	1128
November.	30,125	81,35	8,18	1101
December.	30,087	79,37	1,43	1099

J. GOLDINGHAM.

XII. *On the double Organs of Generation of the Lamprey, the Conger Eel, the common Eel, the Barnacle, and Earth Worm, which impregnate themselves; though the last from copulating, appear mutually to impregnate one another. By Sir* EVERARD HOME, *Bart. V. P. R. S.*

Read February 27, 1823.

In May, 1806, I was so fortunate as to ascertain that the Teredines are hermaphrodites, and that the same individual both formed and impregnated the ova.

In June, 1815, I found the lamprey also to be an animal of the same tribe; and on the present occasion, I wish to explain that the conger eel, the common eel, and the barnacle, are similar in their mode of generation, every one of these animals impregnating itself.

With respect to eels, I am disposed to agree in the opinion of the President of the Society, believing the conger and common eel to belong to the same species; and that the only difference between them is the one living in fresh, the other in salt water, which will explain the difference in their size and colour.

What renders this probable is, that Sir H. DAVY succeeded in getting a fresh water eel to live in salt water; and he understands that, after the eel has been accustomed to salt water for a year, its colours gradually change, acquiring a tint of green. The experiment was carried on in Cornwall, and the eel was sent alive, that we might have the opportunity

of judging for ourselves; but it never arrived in London. He began with young small eels, which all died; when however he took one of a tolerable size, it seemed to suffer little inconvenience.

The organs of generation in the conger and common eel are exactly similar; and I have been so fortunate, on the first of February, 1823, to receive a conger from Plymouth, with ova distinctly visible in the microscope, and the structure of the testicles equally apparent. Out of three congers sent to me, this state of the ova was only met with in one, so that they do not breed regularly in the same month. As this has not, I believe, been noticed before, I got Mr. BAUER to represent the parts in the annexed drawing, and thought it might be satisfactory to the Society to see a magnified drawing of these double organs in the lamprey, where they are brought closer upon one another by the absence of an air bladder, on the sides of which the ovaria of the common eel and conger are spread.

In the eel tribe the kidneys are immediately behind the peritonæum, and so closely connected with the testicle, when the eels are not caught in the breeding season, that the whole mass has been taken for kidney; and had I not been favoured with the assistance of Mr. BAUER and Mr. CLIFT, I might have failed in procuring such accurate and distinct representations of these parts.

Mr. CLIFT, upon a former occasion, made a drawing of the testicles and ovarium of the lamprey, in which the parts were only sufficiently enlarged to identify the facts they were intended to demonstrate. I have now, to establish them more completely, taken advantage of Mr. BAUER's superior skill in

the use of the microscope to give, upon the same plate, the ovarium of the lamprey, and of the conger; wishing in my demonstration of such curious facts, that had escaped the accurate observation of JOHN HUNTER, they should, as far as may be, speak for themselves.

That species of barnacle, called *Lepas Anatifera*, has been examined, and drawings have been made of it by both these great anatomists, HUNTER and CUVIER; but their not having met with it in the breeding season, prevented them from seeing the manner in which the ova are disposed of, and led to considerable error with respect to the organs of generation.

The ovaria are situated round the œsophagus, and may, when not met with in the breeding season, be mistaken for the salivary glands; and the penis may be looked upon as the oviduct for depositing the ova after impregnation.

The curious circumstance in this species of lepas is, that the ova are impregnated before they leave the ovaria, by the point of the penis being bent down and carried for nearly one-fourth of an inch into them. After impregnation, the ova pass through a small opening in the outer covering into the stem by which the body of the barnacle is suspended; in that situation the ova are both defended and supplied with salt water; and, when the embryo is completely formed, it makes its way out, laterally from the stem, leaving behind the shell or covering of the egg attached to the inside of the tube, marking the place from which it escaped, the young lepas acquiring a stem of its own. In some cases the eggs all remain at the root of the stem, and come out externally, just opposite the opening in the outer covering.

All these circumstances are most distinctly illustrated by the drawings.

On the Organs of Generation of the Earth Worm.

It is now six years since Mr. Bauer very kindly offered me his assistance in this enquiry. Our joint labours have been frequently interrupted by subjects of more interest; we have, however, at last completed this investigation, and so much is represented in the drawings, that little is left for verbal description.

The mode of copulation resembles that of the leech more than of the snail; but when the animals are separated there is this curious difference: in the leech, an animal much smaller than the earth worm, there remains protruded a penis an inch long; a little lower down on the belly is the orifice leading to the uterus or ovarium; so that the first impression I received was, that the length of the penis enabled the leech to copulate with itself, till my friend, Dr. Johnson, laid before the Society an account of its copulation with another individual, which he had seen.

When two worms in copulation are forced asunder, there is upon neither of them any appearance of penis or vagina. There is a pair of longitudinal slits or suckers a little way from the head on each side of the belly of both worms; lower down a pair of hooks corresponding to each pair of suckers.

Their mode of copulation is as follows:

Two worms come out of two neighbouring holes a few inches apart, so that there is sufficient space for them to copulate, and for one-third of the length of each worm to remain

in the hole, which enables them to keep their hold of the ground; and whenever they are disturbed, they pull themselves asunder and retreat into their holes. In the act of copulation, as in the leech, the heads lie in opposite directions, so that the hooks of one worm attach themselves to the suckers of the other, which at that time swell out exceedingly, and form a cavity which is filled with mucus. The act of copulation is continued for a considerable time; Mr. BAUER has watched them for several hours before they separated.

The testicles and ovaria (in the breeding season) are shown in the annexed drawings to be very conspicuous in each individual worm; and although no canal from the one to the other has been detected, there can be no doubt that, the semen of the testicles arrives at the ovaria by the coats giving way. The ova, after impregnation, are conveyed into cells, of which there are two rows on each side of the animal, and there deposited.

It is in this situation the ova are hatched; and the young are for some time nourished by a substance supplied from a corrugated canal shown in the drawing, which is met with in the intestine, but having no communication with its cavity; this corrugated canal is firmly attached posteriorly to the parts behind the intestine, and sends off tubes to each of the cells in which the young are hatched: there is also an external orifice leading into each of them. In these cells the young go into the crysalis state, and when the young worm is ready to leave the crysalis covering, which is of an oval form, pointed at each end, one or more of these pointed ends are thrust out at the external orifice, so as to appear

Fig. 1.

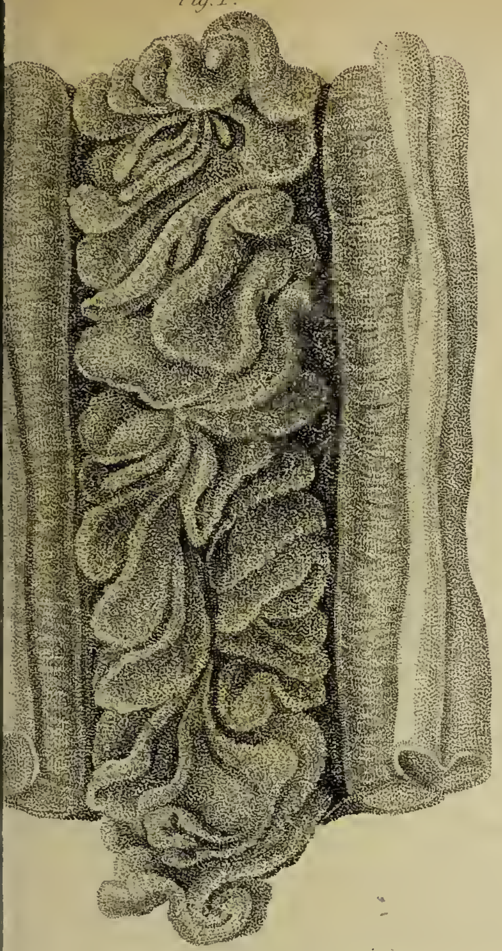


Fig. 3.

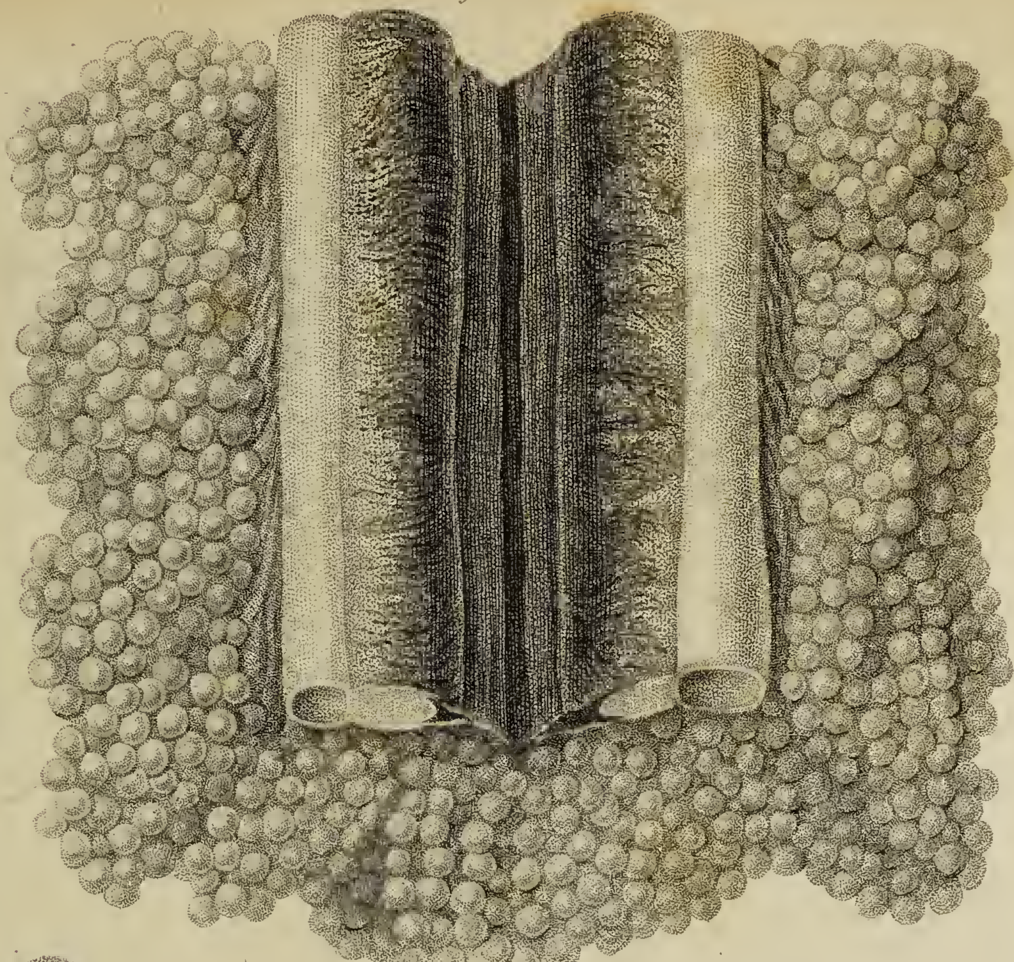


Fig. 2.

Fig. 4.

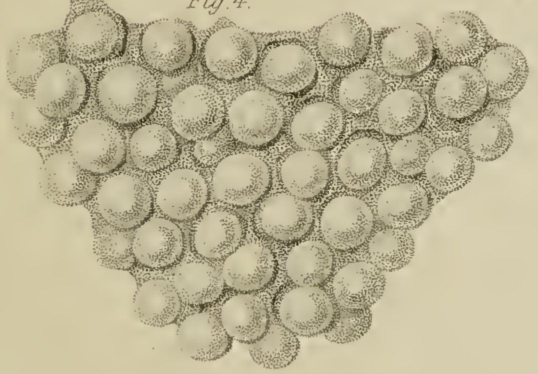


Fig. 5.

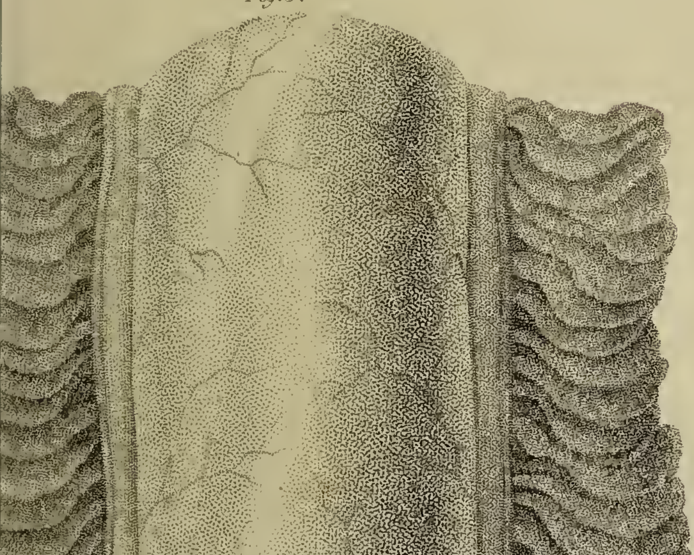


Fig. 6.

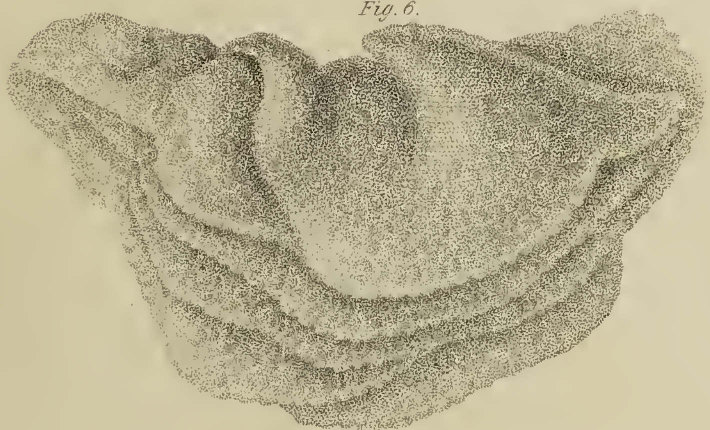
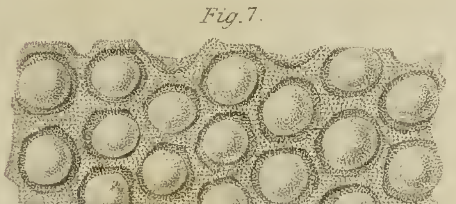


Fig. 7.



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externally, like so many feet for the progressive motion of the worm. The young worm does not remain immured till the crysalis covering is expelled, but eats its way out, leaving the middle broadest part of this covering sticking in the external orifice. The total absence of penis and vagina makes it difficult to explain what purpose is answered by the copulation; from the agitation produced by it through the whole of the bodies of the two worms, it seems necessary to distribute the ova to the numerous decipimenta in which they are met with.

This is somewhat different from the leech, whose ova are deposited in lateral cells, and are squeezed out at lateral orifices, covered with a mass of mucus, which at first encloses the whole leech. The animal disengages its head by means of the teeth, and then, by the wriggling of the body, escapes, leaving the mass of eggs and mucus curled up with a hole through it in the form of a ball, which the leech deposits upon the water, and there the young go through the crysalis state, and eat their way out and provide for themselves.

EXPLANATION OF THE PLATES.

Plate XV. The Lamprey.

Fig. 1. Anterior view of a portion of the ovarium and testicles of the lamprey, before the breeding season; magnified two diameters.

Fig. 2. A small portion of the same ovarium; magnified five diameters.

Fig. 3. Posterior view of a portion of the ovarium and testicles of the lamprey, in the breeding season; magnified two diameters.

Fig. 4. A small portion of the same ovarium; magnified five diameters.

Fig. 5. Anterior view of a portion of the ovarium, testicles, and the air bladder of the conger; natural size.

Fig. 6. A small portion of the same ovarium; magnified five diameters.

Fig. 7. A very small portion of the same; magnified fifty diameters.

PLATE XVI. The EARTH WORM.

Fig. 1. Exhibits a posterior or back view of a dead worm, in which state it is contracted; magnified two diameters.

Fig. 2. An internal view of the same worm laid open from behind; magnified two diameters.

Fig. 3. The upper portion of the same parts; magnified four diameters. In the middle line lies the great artery and the six lateral cells carrying red blood, and communicating between the great arterial and trunk, and the venal one on the opposite side or belly: the artery passes up to the head on the outside of the glandular mass surrounding the œsophagus, and through the space between the three portions of which the brain is composed, to go down the opposite or belly side of the animal. Immediately on the outside of the three lowest arterial cells are the ovaria and testicles. As the animal is divided through its whole length by decipimenta into compartments, one ovarium and one testicle lies on each side of the same compartment with the fourth arterial cell, and one testicle and ovarium in the same compartment with the fifth. In the sixth, there is an ovarium but no testicle. In the compartment below the arterial cells is one ovarium on each side very much developed, and directly between them is a hard cartilaginous circle, through which the œsophagus passes.

Fig. 1.

Fig. 2.

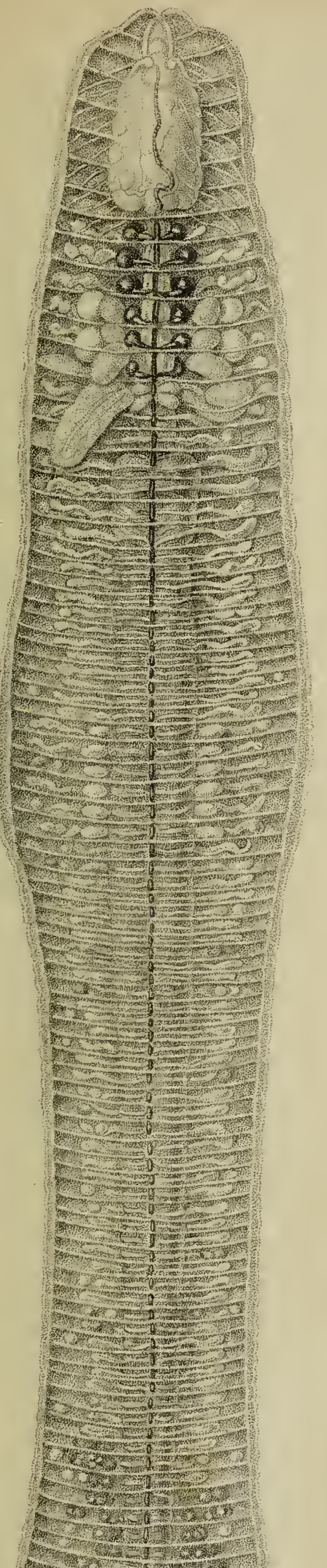
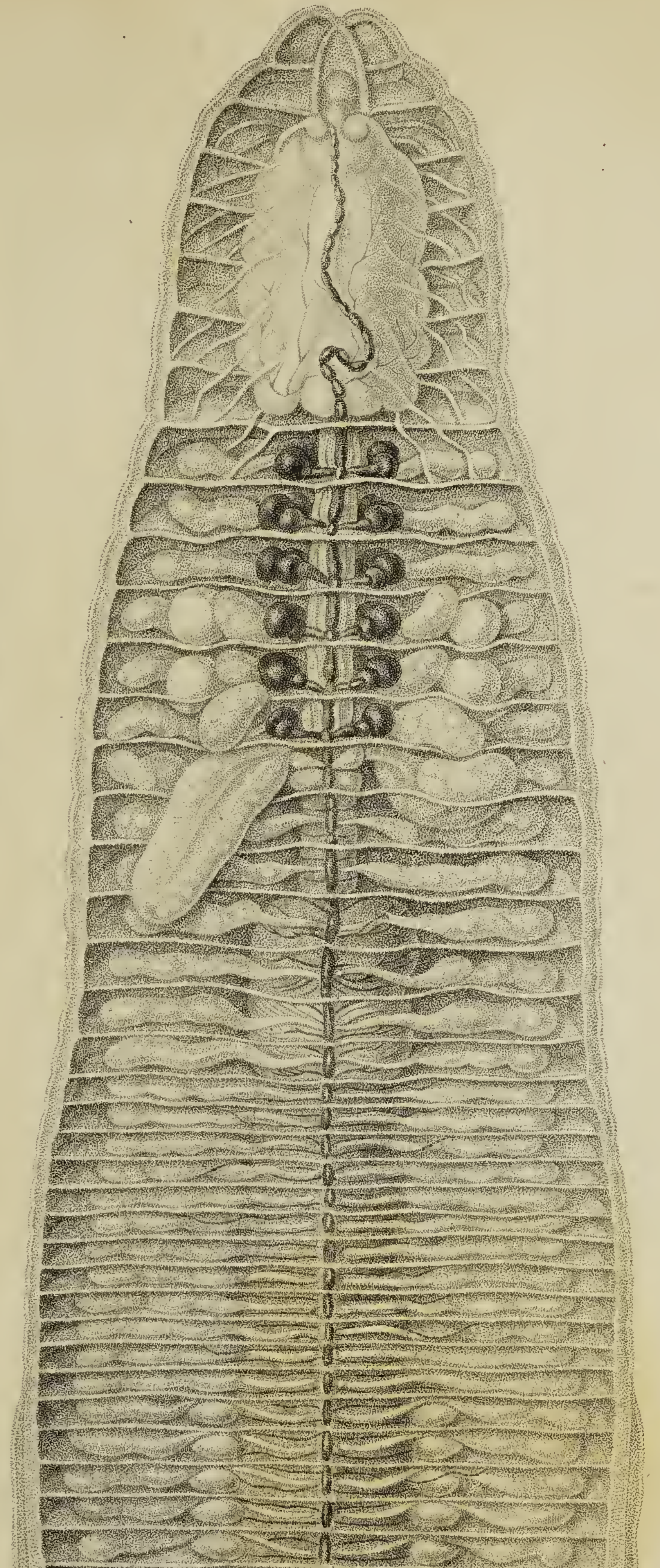


Fig. 3.



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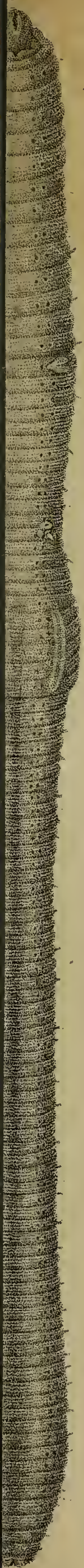


Fig. 2.

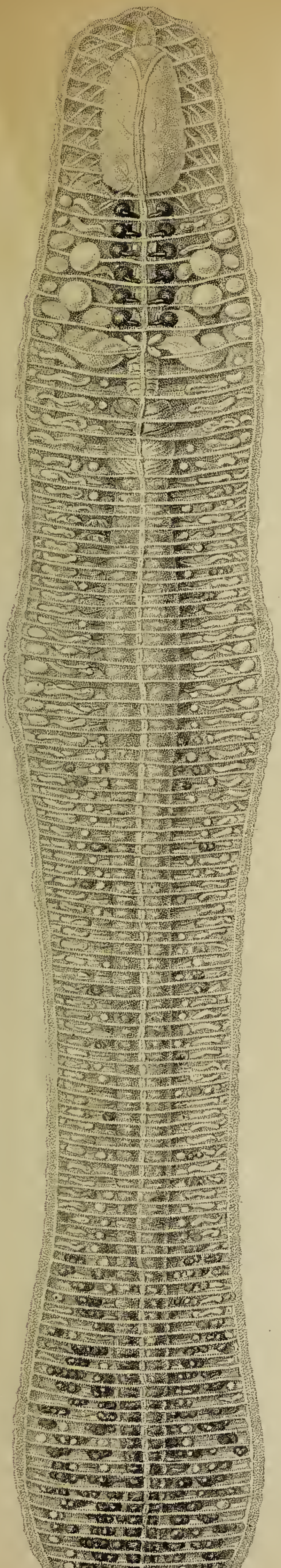
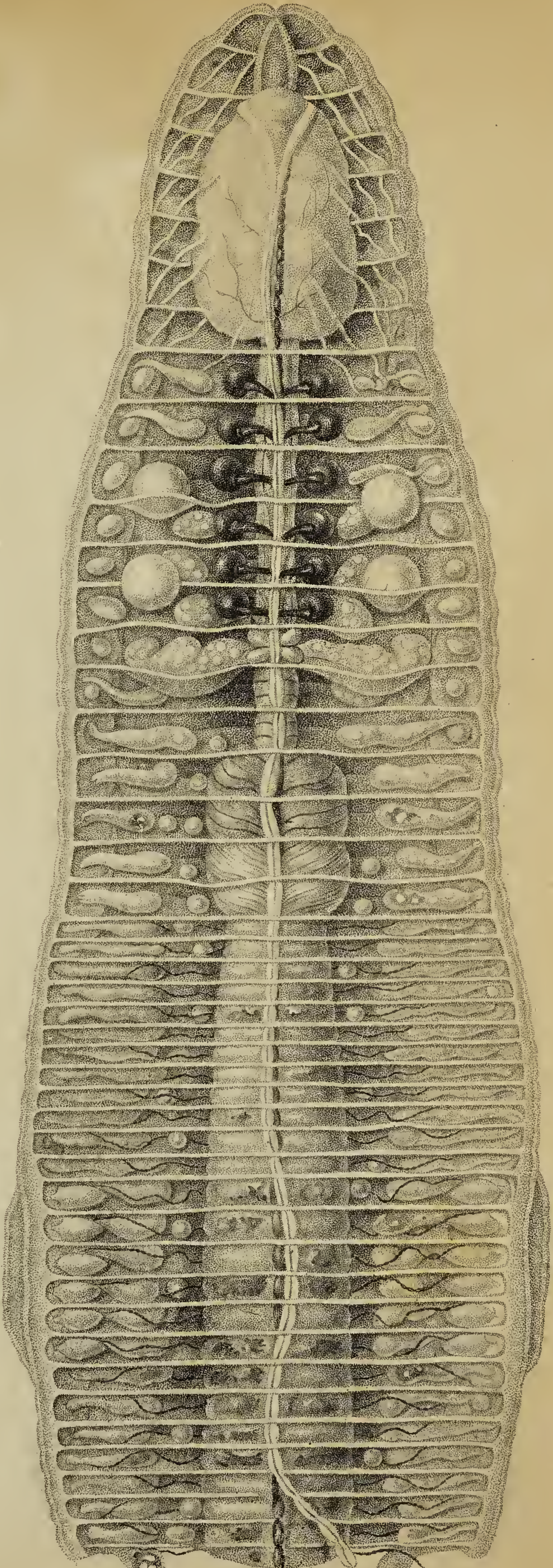


Fig. 3.



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The other compartments contain membranous bags for the reception of ova. The two lowest ovaria are hanging loose. Before the breeding season they are empty. Towards the lower part, on each side, is the cut edge of the lateral slit, by means of which the animals adhere in the act of copulation.

In this space are included six compartments: in each of these is a large conspicuous gland of an oval form.

The course of the œsophagus, crop, gizzard, and intestine, can be traced behind the great artery.

PLATE XVII. The EARTH WORM.

Fig. 1. An anterior or belly view of the perfect worm after death; magnified two diameters.

Fig. 2. The internal parts of the same worm: magnified two diameters.

Fig. 3. The same parts for a certain extent; magnified four diameters.

At the upper part is seen the brain, on each side of which go down two nervous elongations from two spherical ganglions situated on the opposite side to form the spinal marrow, which in this animal runs along the belly.

The spinal marrow is traced through its course, showing the artery belonging to its coats.

Many parts already described are seen in this view.

The lower developed ovaria appear in this view suspended upon the decipimentum. Immediately below them the termination of the œsophagus is seen, under which is the crop, then the gizzard, leading to the intestine.

PLATE XVIII. The EARTH WORM.

Fig. 1. The same section as fig. 2, Plate XVII. magnified two diameters, showing the more internal parts for which purpose the spinal marrow is turned on one side: the intestine is laid open, exposing an hexangular tube, which has no communication with the intestine itself, but is posteriorly attached to that canal, and has two lateral openings into each compartment, showing the hexangular tube to be a reservoir of nutriment for the young.

Fig. 2. A posterior view of the outside of a portion of the intestine; magnified eight diameters.

Upon this portion of intestine are seven partial contractions formed by the transverse decipimenta, between each of which are two orifices leading to the lateral membranous bags from the hexagonal tube.

Between the two rows of openings is a longitudinal passage, forming a communication between the ovaria and the cells in the different compartments.

Fig. 3. A transverse section of a portion of fig. 2, seen anteriorly.

Fig. 4. A portion of intestine magnified as in fig. 2, laid open anteriorly to show the hexangular tube.

Fig. 5. The external appearance of the mouth and head; magnified six diameters.

Fig. 6. Shows the external appearance of the parts connected together in the time of copulation; magnified six times.

In the second ring from the top are two hemispherical protuberances with transverse slits.

Fig. 1.

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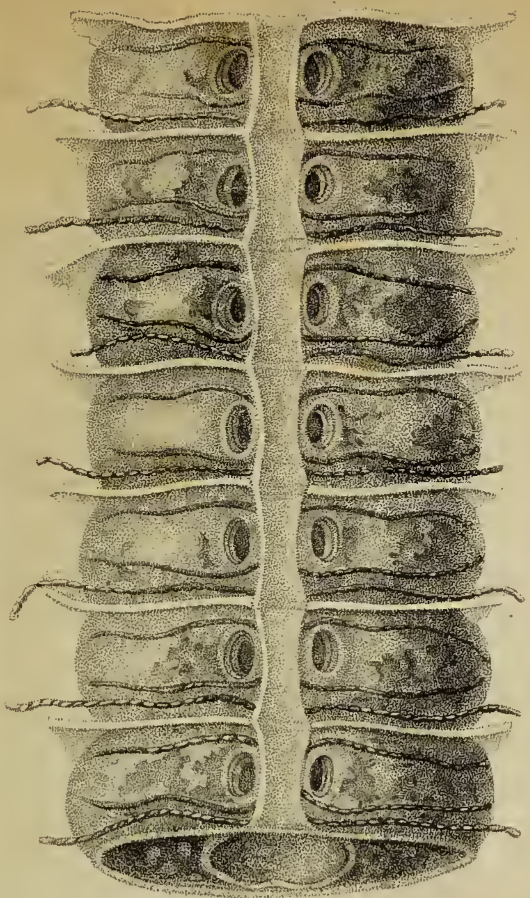

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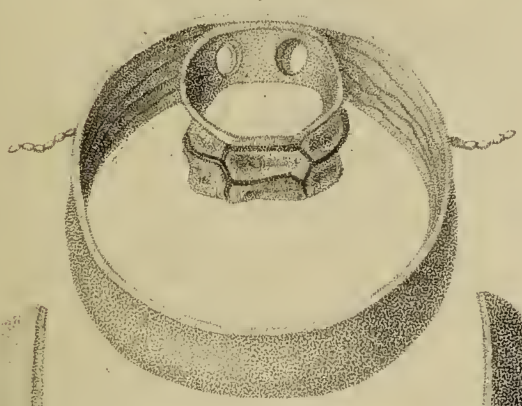

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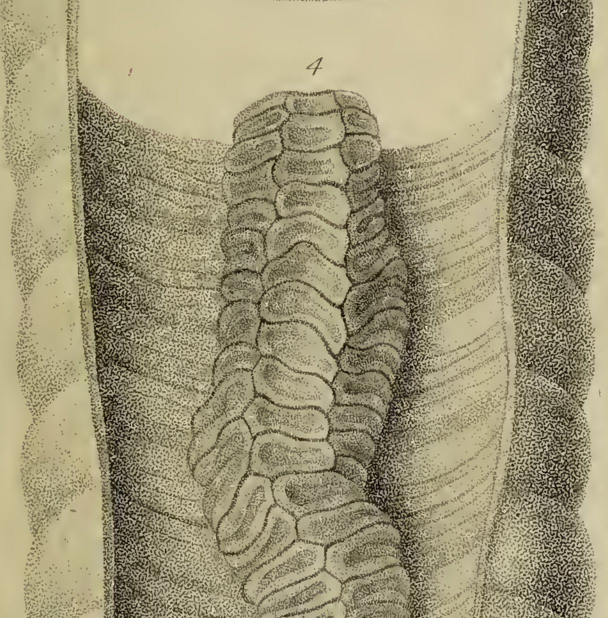

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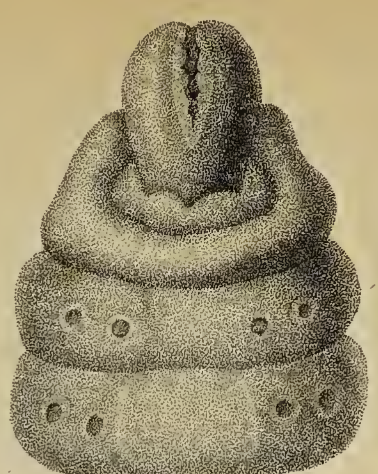

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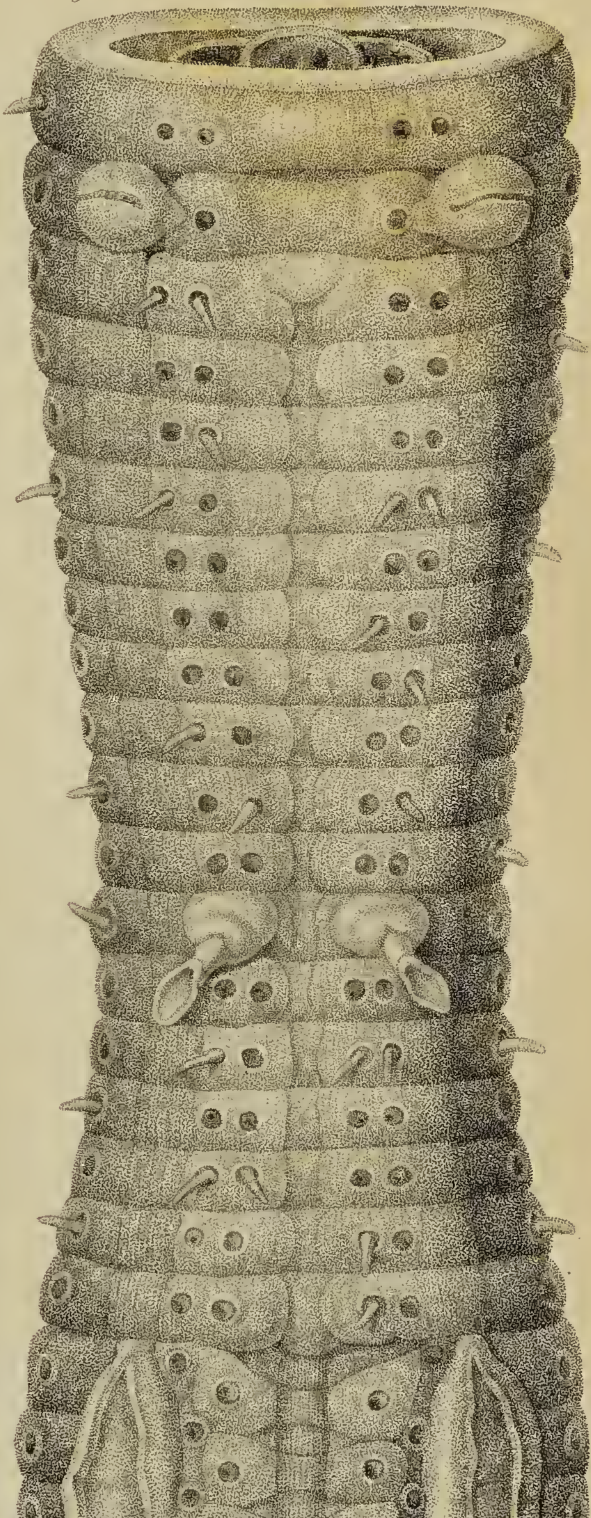


Jˢ. Basire sculpᵗ.

Fig.1.

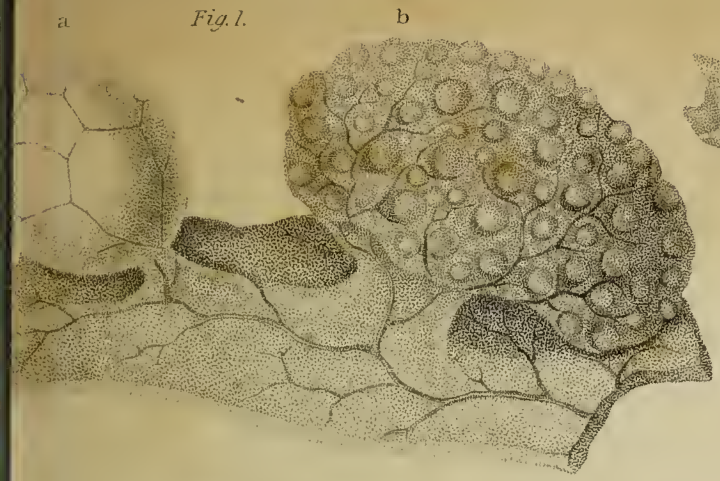


Fig.3.

Fig.2.

Fig.5.

Fig.4.

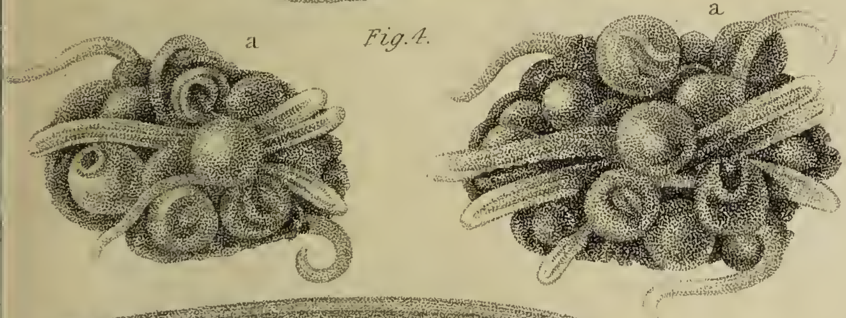


Fig.6.

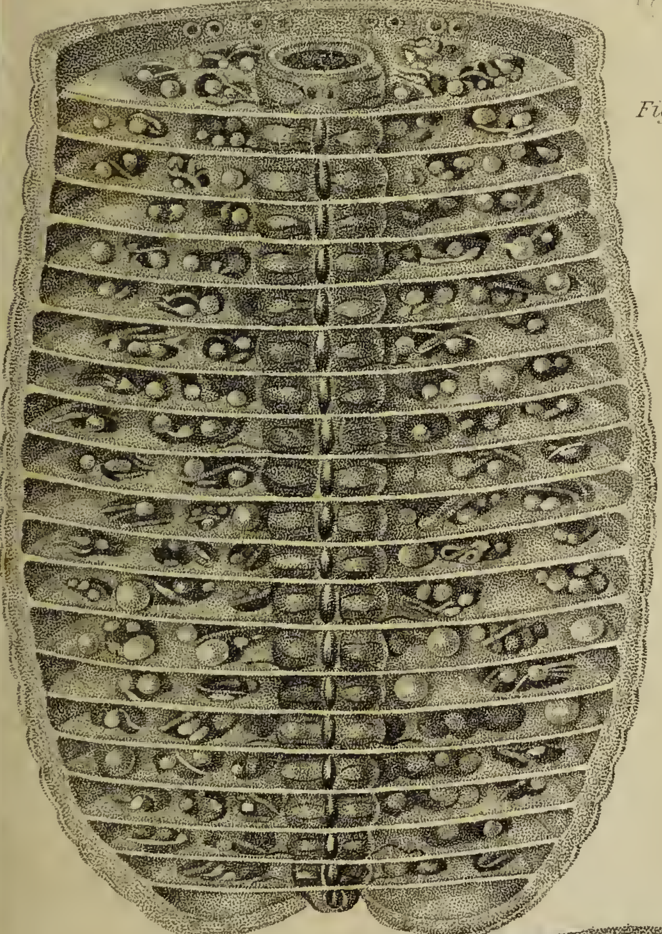


Fig.7.

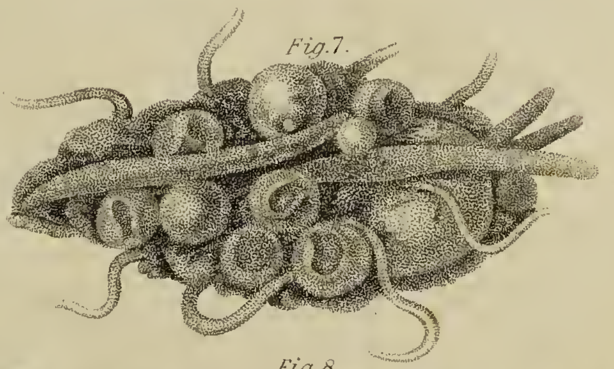
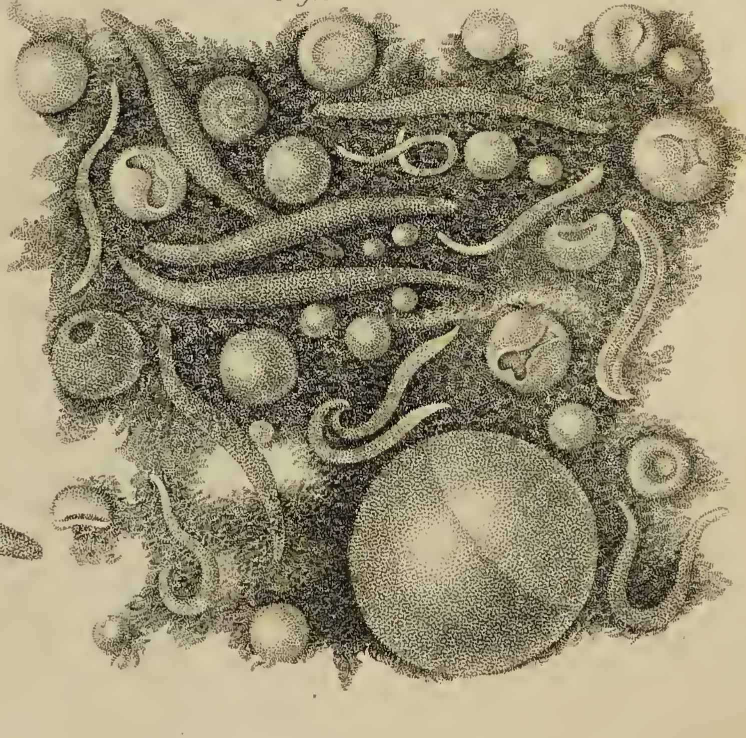


Fig.8.



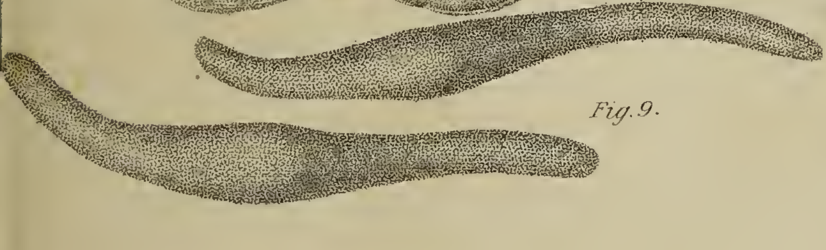


Fig.9.

uer, del^t.

J^s Basire sculp^t.

In the eleventh ring are two protuberances, from which, during copulation, two spoon-like suckers are projected, but at other times withdrawn.

At the eighteenth ring begin the longitudinal lateral slits, and they are continued to the twenty-second. These are shown in an expanded state, in which they are only met with at the moment of the coitus.

In this view the openings from the compartments are distinctly seen. On the middle line of the belly are two rows, and other two rows on each side; these last are only distinctly seen in this drawing where the crysales are projecting through them.

PLATE XIX. The EARTH WORM.

Fig. 1. A testicle and ovarium at the breeding season in situ; magnified twelve times.

Fig. 2. Ova taken from the ovarium; magnified fifty times.

Fig. 3. One of the membranous bags filled with eggs and worms removed from the compartment which contained it; magnified fifteen times.

a a a. Eggs ready to hatch; magnified fifty times.

b b b. Eggs hatching; magnified fifty times.

c c. Two embryos hatched.

Fig. 4. A membranous bag taken from a compartment lower down in the worm; magnified fifteen times.

a a. Clusters of eggs and embryos agglutinated together; magnified thirty times.

Fig. 5. *a a.* The embryo in motion just before going into the crysalis state; magnified thirty times.

b. The change into the crysalis begun; magnified thirty times.

cc. The same, magnified fifty times.

Fig. 6. The tail portion of a worm opened from behind; magnified four times.

To show that the ova and membranous bags are not met with at the lowest part of the worm, the compartments containing clusters of agglutinated ova, embryos, and crysales.

Fig. 7. One of the clusters of the last figure; magnified thirty times.

Fig. 8. The same cluster kept in water for fifteen minutes, and its contents exposed.

Fig. 9. Two perfect crysales from the same cluster; magnified fifty times.

PLATE XX. BARNACLES.

Fig. 1. A portion of the pedicle, with the shell from which the left valve is removed.

Fig. 2. A Longitudinal section of Fig. 1, to show the eggs within the fibrous substance of the pedicle; and the animal removed, to show the mantle in its natural position lining the shell; natural size.

Fig. 3. The entire animal taken out of the shell, and the mantle or lining of the shell turned back; magnified two diameters.

Fig. 4. The animal with the external membrane turned back, to show the eggs which accumulate there before they pass into the pedicle; magnified two diameters. In these two figures the tentacula of the right side are removed, to prevent confusion.

Fig. 5. The animal with all its integuments removed, and also the tentacula of the left side removed to show the natural position of the penis; magnified two diameters.

J.ˢ Basire sculp.ᵗ

Fig. 6. A front view of the animal's head ; magnified four diameters.

A. External jaw.

B. Middle jaw

C. Internal jaw.

Fig. 7. Side view of the same ; magnified four diameters.

Fig. 8. One of the tentacula ; magnified four diameters.

Fig. 9. The penis ; magnified four diameters.

Fig. 10. A small portion of the eggs which are accumulating under the external integuments of the animal ; magnified fifty diameters.

Fig. 11. A small portion of the fibrous substance and eggs from within the pedicle ; magnified fifty diameters.

Fig. 12. Some separate eggs ; magnified one hundred diameters.

Fig. 13. The membrane lining the shell at the base of the pedicle, and of which the mantle is a continuation ; magnified two diameters.

Fig. 14. One of the outer jaws seen in fig. 6.

B. The middle jaw.

C. Internal jaw.

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METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1822.

1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Jan. 1	8	0	38	48	29,84	36	Rain this Month 0,215 Inches.	S	1	Cloudy and hazy.
	2	0	44	53	29,64	45		W	1	Fine.
2	8	0	45	48	29,61	45		W	1	Fine.
	2	0	41	52	29,64	45		SSW	1	Cloudy and hazy.
3	8	0	35	49	29,81	35		W	1	Hazy.
	2	0	40	51	29,70	41		S	1	Cloudy.
4	8	0	37	48	29,10	36		E	1	Rain.
	2	0	39	51	29,45	41		WSW	1	Cloudy and Rain at intervals.
5	8	0	35	47	29,77	36		N	1	Fine.
	2	0	39	51	29,91	41		ESE	1	Cloudy.
6	8	0	35	47	29,99	35		N	1	Cloudy.
	2	0	37	46	30,02	40		E	1	Fine.
7	8	0	34	45	29,92	33		NNW	1	Hazy thick weather.
	2	0	36	48	29,98	37		ESE	1	Cloudy.
8	8	0	34	46	30,08	35		SSW	1	Cloudy.
	2	0	40	48	30,09	40		N	1	Cloudy.
9	8	0	40	47	30,13	40		N	1	Hazy and cloudy.
	2	0	44	50	30,16	45		N	1	Fine.
10	8	0	35	41	30,13	34		W	1	Fine, rather hazy.
	2	0	42	51	30,13	45		W	1	Fine.
11	8	0	42	49	30,20	40		W	1	Cloudy and hazy.
	2	0	47	53	30,24	48		NW	1	Cloudy.
12	8	0	42	50	30,31	41		W	1	Cloudy.
	2	0	48	54	30,31	48		W	1	Cloudy.
13	8	0	45	50	30,30	44		W	1	Fine.
	2	0	49	54	30,29	49		W	1	Cloudy.
14	8	0	42	50	30,26	42		W	1	Fine.
	2	0	47	54	30,26	49		W	1	Fine.
15	8	0	40	49	30,12	40		N	1	Fine.
	2	0	43	53	30,20	47		N	1	Fine.
16	8	0	34	46	30,18	34		WNW	1	Hazy.
	2	0	36	49	30,13	42		WNW	1	Fine.

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for January, 1822.

1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Jan. 17	8	0	32	45	30,10	31	Rain this Month 0,215 Inches.	W	1	Cloudy; snow in the night.
	2	0	38	49	30,12			W	1	Rain.
18	8	0	35	48	30,29	35		W	1	Foggy.
	2	0	43	53	30,34	45		W	1	Cloudy.
19	8	0	41	48	30,35	35		W	1	Cloudy and foggy.
	2	0	46	51	30,37	46		W	1	Cloudy.
20	8	0	45	50	30,22	44		W	1	Cloudy.
	2	0	49	49	30,29	50		W	1	Cloudy.
21	8	0	40	47	30,28	39		W	1	Cloudy and hazy.
	2	0	47	54	30,30	50		NW	1	Fine.
22	8	0	43	50	30,43	43		W	1	Cloudy and hazy.
	2	0	48	54	30,45	48		W	1	Cloudy.
23	8	0	42	51	30,32	42		SSW	1	Fine.
	2	0	48	54	30,25	48		W	1	Cloudy.
24	8	0	43	51	29,85	42		S	1	Rain.
	2	0	49	55	29,86	49		W by S	1	Cloudy.
25	8	0	44	53	29,96	44		WNW	1	Cloudy.
	2	0	48	55	30,00	49		N	1	Cloudy.
26	8	0	43	52	30,10	42		W	1	Cloudy.
	2	0	48	54	30,09	49		W	1	Fine.
27	8	0	36	51	30,28	36		W	1	Fine.
	2	0	43	51	30,31	49		S	1	Cloudy damp weather.
28	8	0	46	50	30,21	48		W	1	Hazy.
	2	0	50	58	30,17	50		W	1	Fine.
29	8	0	38	51	30,18	38		W	1	Fine.
	2	0	48	53	30,19	51		W	1	Cloudy.
30	8	0	36	51	30,32	36		W	1	Hazy.
	2	0	44	53	30,34	49		W	1	Fine.
31	8	0	34	48	30,33	31		W	1	Fine; rather hazy.
	2	0	43	53	30,30	44		W by S	1	Fine.

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1822	Time.	Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H. M.	°	°	Inches.			Points.	Str.	
Feb. 1	8 0	41	51	30,23	39	Rain this Month 0,560 Inches.	SW	1	Fine.
	2 0	45	57	30,13	46		SW	1	Fine.
2	8 0	45	52	29,84	39		SW	1	Cloudy.
	2 0	50	55	29,71	51		W	2	Cloudy.
3	8 0	45	52	29,42	45		W	1	Fine.
	2 0	47	52	29,46	52		W	1	Fine.
4	8 0	37	49	29,74	36		W	1	Fine : hoar frost.
	2 0	46	55	29,68	48		S	1,2	Cloudy.
5	8 0	47	52	29,30	41		SW	3	Rain.
	2 0	50	55	29,30	54		W	1	Rain.
6	8 0	35	49	30,11	34		W	1	Fine.
	2 0	42	59	30,16	50		S	1	Fine.
7	8 0	44	51	29,87	41		S	2	Fine.
	2 0	50	55	29,84	51		S	2	Cloudy.
8	8 0	45	53	29,76	44		SW	2	Cloudy.
	2 0	49	57	29,91	51		SW	1	Cloudy.
9	8 0	45	52	29,99	44		S	2	Cloudy.
	2 0	51	57	29,85	53		W	1	Cloudy.
10	8 0	47	53	29,85	47		SSE	1	Cloudy.
	2 0	52	55	29,83	53		SE	1	Fine.
11	8 0	42	52	29,96	42		SW	1	Fine.
	2 0	49	58	30,05	52		SSW	1	Fine.
12	8 0	41	55	30,23	41		W	1	Thick fog.
	2 0	46	57	30,23	50		ESE	1	Fog.
13	8 0	39	53	30,13	36		E	1	Foggy.
	2 0	49	58	30,09	49		S	1	Fine.
14	8 0	43	53	30,11	42		SSE	1	Cloudy.
	2 0	50	56	30,10	50		S	1	Fine.
15	8 0	43	56	30,09	42		S	1	Fine.
	2 0	52	60	30,09	53		SSW	1	Fine.

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1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Feb. 16	8	0	40	54	30,36	40	Rain this Month 0,560 Inches.	W	1	Hazy.
	2	0	47	59	30,41	52		W	1	Fine.
17	8	0	47	55	30,41	45		W	1	Cloudy.
	2	0	53	57	30,40	50		W	1	Fine.
18	8	0	45	55	30,40	42		WSW	1	Cloudy.
	2	0	53	59	30,37	54		S	1	Fine and clear.
19	8	0	45	56	30,42	44		N	1	Damp and foggy.
	2	0	50	59	30,38	51		W	1	Cloudy.
20	8	0	42	55	30,10	39		W	1,2	Cloudy.
	2	0	48	56	29,92	50		W	2	Cloudy.
21	8	0	37	52	30,37	36		NNW	1	Fine.
	2	0	47	56	30,43	45		NW	1	Fine.
22	8	0	37	54	30,44	36		S	1	Fine.
	2	0	46	60	30,32	48		W	1	Fine.
23	8	0	42	55	30,25	40		W	1	Hazy.
	2	0	46	58	30,29	46		WNW	1	Fine.
24	8	0	43	54	30,22	41		W	1	Cloudy.
	2	0	52	54	30,15	54		NNW	1,2	Cloudy.
25	8	0	48	54	30,24	48		W	1,2	Cloudy.
	2	0	53	56	30,23	54		W	1.	Cloudy.
26	8	0	44	53	30,12	44		W	1,2	Fine.
	2	0	53	58	30,06	54		W	1	Cloudy.
27	8	0	39	53	30,50	38		W	1	Fine; rather hazy.
	2	0	47	56	30,63	49		N	1	Fine.
28	8	0	37	54	30,69	35		W	1	Hazy thick weather.
	2	0	46	59	30,58	48		S	1	Fine.

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March, 1822.

1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Mar. 1	7	0	36	54	30,28	35	Rain this Month 1,273 Inches.	SE	1	Thick Fog.
	2	0	46	60	30,20	47		W	1	Fine.
2	7	0	45	55	30,29	42		SW	1	Cloudy and hazy.
	2	0	53	57	30,28	53		S	1,2	Cloudy.
3	7	0	43	53	30,29	43		SW	1	Fine.
	2	0	53	56	30,24	54		S	1	Fine.
4	7	0	43	53	30,09	42		SW	1	Fine.
	2	0	53	57	29,99	53		SW	1	Fine.
5	7	0	47	55	29,95	47		W	1	Cloudy.
	2	0	53	60	29,99	54		W	1	Fine.
6	7	0	50	58	29,56	46		S	2,3	Rain.
	2	0	55	58	29,57	55		W	1	Cloudy.
7	7	0	43	56	29,57	42		W	1	Fine.
	2	0	47	59	29,44	52		W	1	Rain.
8	7	0	39	54	29,68	38		W	1	Cloudy.
	2	0	48	59	29,43	49		W	1	Rain.
9	7	0	44	55	29,69	43		W	1	Fine.
	2	0	53	60	29,69	54		W	1	Rain.
10	7	0	50	57	29,59	50		SW	1	Cloudy.
	2	0	57	58	29,56	57		W	1,2	Fine.
11	7	0	42	52	29,78	40		W	1	Fine.
	2	0	47	58	29,97	52		NW	1,2	Cloudy.
12	7	0	40	53	30,36	35		W	1	Hazy.
	2	0	48	56	30,36	48		SW	1	Fine.
13	7	0	41	53	30,21	40		SSE	1	Fine.
	2	0	51	57	30,05	51		E	1,2	Fine.
14	7	0	47	54	29,91	45		SSW	1	Cloudy.
	2	0	56	57	29,97	57		SW	1	Rain.
15	7	0	41	55	30,22	41		NW	1	Cloudy and hazy.
	2	0	58	59	30,26	58		W	1	Fine.
16	7	0	45	55	30,29	42		S	1,2	Fine.
	2	0	56	59	30,13	56		SSE	1	Cloudy.

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	H.	M.	°	°	Inches.			Points.	Str.	
Mar. 17	7	0	51	56	30,27	50	Rain this Month 1,273 Inches.	W	1	Cloudy.
	2	0	54	57	30,19	56		SW	1	Rain.
18	7	0	48	57	30,23	48		NW	1	Cloudy.
	2	0	56	57	30,30	56			1	Cloudy.
19	7	0	50	57	30,28	46		SW	1	Cloudy.
	2	0	60	61	30,26	61		W	2	Cloudy.
20	7	0	51	57	30,32	50		W	1	Cloudy.
	2	0	58	60	30,25	60		W	1	Fine.
21	7	0	45	55	30,27	45		SW	1	Cloudy.
	2	0	58	60	30,15	58		W	1	Fine.
22	7	0	45	56	30,30	44		W	1	Fine.
	2	0	56	61	30,35	58		WNW	1	Fine.
23	7	0	42	54	30,24	40		W	1	Fine.
	2	0	59	60	30,06	59		SW	1	Fine.
24	7	0	48	57	29,60	46		W	1	Cloudy.
	2	0	52	56	29,61	60		NW	1,2	Cloudy.
25	7	0	42	51	29,74	38		W	1	Cloudy.
	2	0	47	56	29,70	52		W by N	1	Rain.
26	7	0	43	53	30,04	38		WSW	1	Cloudy.
	2	0	57	58	30,11	58		NW	1	Cloudy.
27	7	0	50	56	30,20	49		W	1	Cloudy.
	2	0	58	60	30,17	59		S	1	Fine.
28	7	0	50	58	29,94	49		SW	1	Fine.
	2	0	65	63	29,92	66		SW	1	Fine.
29	7	0	47	57	30,38	45		W	1	Fine.
	2	0	62	62	30,39	65		W	1	Fine.
30	7	0	49	56	29,68	47		SW	2	Cloudy.
	2	0	53	61	29,46	55		W	2	Cloudy.
31	7	0	40	51	30,33	38		NE	2	Fine.
	2	0	42	52	30,35	47		NE	2	Fine.

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1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
April 1	7	0	40	50	30,34	38	Rain this Month 1,620 Inches.	N	1	Cloudy.
	2	0	47	56	30,32	48		N	1	Cloudy.
2	7	0	43	51	30,30	41		N	1	Cloudy.
	2	0	49	57	30,31	50		N	1,2	Fine.
3	7	0	43	52	30,40	42		N	1	Cloudy.
	2	0	53	56	30,35	52		NW	1	Cloudy.
4	7	0	45	54	30,24	43		N	1	Cloudy.
	2	0	52	58	30,16	53		NW	1	Cloudy.
5	7	0	45	55	30,05	44		NW	1	Cloudy.
	2	0	54	55	30,00	54		N	1	Cloudy.
6	7	0	45	52	29,89	47		N	1	Fine.
	2	0	54	57	29,85	54		NNE	1	Fine.
7	7	0	43	52	29,95	40		N	1	Cloudy.
	2	0	49	54	29,95	53		W	1	Cloudy, but pleasant.
8	7	0	41	52	30,02	39		N	1	Fine.
	2	0	46	58	30,03	50		N	1	Cloudy.
9	7	0	37	50	30,09	35		N	1	Cloudy.
	2	0	46	56	30,06	48		N	1	Cloudy.
10	7	0	37	48	30,04	36		N	1	Cloudy.
	2	0	46	54	30,01	47		ESE	1	Fine.
11	7	0	39	50	30,01	37		E	1	Fine.
	2	0	48	58	29,90	48		E by S	1	Fine.
12	7	0	41	52	29,72	41		E	1	Rain.
	2	0	53	57	29,72	53		N	1	Cloudy.
13	7	0	45	54	29,85	44		SE	2	Rain.
	2	0	58	60	29,92	58		S	1	Fine.
14	7	0	46	53	30,03	46		SE	1	Cloudy and foggy.
	2	0	57	56	29,96	59		E	1	Cloudy, but pleasant.
15	7	0	50	54	30,00	49		E	1	Fine.
	2	0	63	59	29,98	61		E	1	Cloudy.

METEOROLOGICAL JOURNAL

for April, 1822.

1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Apr. 16	7	0	49	56	29,94	49	Rain this Month 1,620 Inches.	N	1	Rain.
	2	0	54	58	29,99	55		N	1	Cloudy.
17	7	0	43	57	29,92	43		E	1	Cloudy and dark.
	2	0	53	61	29,80	55		E	1	Fine.
18	7	0	46	56	29,66	46		NE	1	Cloudy.
	2	0	51	59	29,64	52		E	1	Cloudy.
19	7	0	44	55	29,68	46		W	1	Cloudy.
	2	0	54	60	29,71	55		S	1	Fine.
20	7	0	44	55	29,69	45		W	1	Fine.
	2	0	55	60	29,69	56		W by S	1	Cloudy.
21	7	0	46	57	29,61	46		W	1	Cloudy.
	2	0	57	57	29,49	58		SW	1	Cloudy.
22	7	0	47	56	29,37	46		S	2	Cloudy.
	2	0	58	62	29,37	58		S	1	Fine.
23	7	0	48	57	29,30	47		S	2	Cloudy.
	2	0	55	62	29,41	56		W	1	Fine.
24	7	0	48	54	29,62	44		S	2	Fine.
	2	0	58	60	29,61	59		S	2	Fine.
25	7	0	47	57	29,56	47		S	2	Rain.
	2	0	57	60	29,58	58		W	1	Cloudy.
26	7	0	46	55	29,82	43		W	1	Fine.
	2	0	56	60	29,95	58		W	1	Fine.
27	7	0	48	57	29,99	47		SW	1	Rain.
	2	0	59	61	30,01	59		SW	1	Cloudy.
28	7	0	51	58	30,25	50		W	1	Cloudy.
	2	0	63	59	30,25	63		SW	1	Cloudy.
29	7	0	48	57	30,23	47		E	1	Cloudy and hazy.
	2	0	63	62	30,22	64		E	1	Fine.
30	7	0	50	59	30,24	48		E	1	Fine.
	2	0	63	63	30,26	64		E	1	Fine clear sky.

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1822	Time.	Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H. M.	°	°	Inches.			Points.	Str.	
May 1	7 0	50	59	30,31	48	Rain this Month 1,022 Inches.	W	1	Fine clear sky.
	2 0	62	61	30,27	64		E	1	Fine and clear.
2	7 0	50	59	30,17	46		E	1	Fine.
	2 0	62	62	30,09	62		E	1	Fine.
3	7 0	47	59	29,96	45		E	1	Fine, rather hazy.
	2 0	62	62	29,86	62		E	1	Fine.
4	7 0	54	60	29,61	53		E	1	Fine.
	2 0	65	63	29,59	66		E	1	Fine.
5	7 0	53	60	29,68	52		NE	1	Rain.
	2 0	66	63	29,70	66		E	1	Fine.
6	7 0	57	61	29,73	56		E	1	Cloudy and hazy.
	2 0	67	64	29,72	68		N	1	Cloudy.
7	7 0	59	62	29,76	59		E	1	Cloudy and hazy.
	2 0	65	62	29,78	68		NE	1	Rain.
8	7 0	49	59	29,95	47		NE	1	Cloudy.
	2 0	54	61	29,94	56		E	1	Fine.
9	7 0	49	55	29,74	47		E	1	Cloudy.
	2 0	54	60	29,61	55		E	1	Cloudy.
10	7 0	53	57	29,38	51		E	1	Cloudy and hazy.
	2 0	55	54	29,30	55		E	1	Fine.
11	7 0	47	58	29,56	45		E	1	Fine.
	2 0	61	59	29,65	62		E	1	Cloudy.
12	7 0	51	55	29,79	51		N	1	Cloudy.
	2 0	51	54	29,77	52		N	1	Cloudy.
13	7 0	51	55	29,82	50		N	1	Cloudy and rather hazy.
	2 0	57	58	29,82	58		N	1	Cloudy.
14	7 0	50	57	29,87	47		N by E	1	Fine.
	2 0	60	60	29,87	60		N	1	Fine.
15	7 0	60	58	29,91	59		E	1	Fine.
	2 0	68	61	29,81	68		NE	1	Fine.
16	7 0	52	59	29,98	50		E	1	Fine.
	2 0	62	62	29,95	62		E	1	Fine; no cloud to be seen.

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1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
May 17	7	0	61	60	29,95	61	Rain this Month 1,022 Inches.	N by E	1	Fine.
	2	0	70	65	29,92	72		N	1	Fine.
18	7	0	62	63	29,99	60		N	1	Fine; rather hazy.
	2	0	70	67	30,01	72		N	1	Fine.
19	7	0	60	63	30,07	56		NNE	1	Fine.
	2	0	72	65	30,04	72		N	1	Fine.
20	7	0	60	64	30,13	56		E	1	Hazy.
	2	0	74	69	30,11	75		E	1	Fine.
21	7	0	60	65	30,25	58		N	1	Fine.
	2	0	74	70	30,25	74		NE	1	Fine.
22	7	0	55	64	30,46	53		N	1	Fine.
	2	0	70	68	30,30	70		E	1	Fine.
23	7	0	55	63	30,27	50		NE	1	Cloudy.
	2	0	63	65	30,19	65		NE	1	Fine.
24	7	0	56	63	30,08	52		E	1	Cloudy.
	2	0	64	64	30,02	65		NE	1	Fine.
25	7	0	54	63	29,96	50		E	1	Fine. [from 1½ till 2½ P.M.
	2	0	61	63	29,88	65		W	1	Thunder and lightning
26	7	0	56	62	29,80	53		W	1	Cloudy.
	2	0	55	61	29,78	60		SW	1,2	Rain.
27	7	0	53	60	30,11	51		S	1	Fine.
	2	0	64	64	30,06	65		W	1	Fine clear sky.
28	7	0	61	62	30,17	60		W	1	Cloudy.
	2	0	68	64	30,20	69		NNW	1	Fine.
29	7	0	58	62	30,24	56		W	1	Fine.
	2	0	71	65	30,21	71		W	1	Fine.
30	7	0	55	62	30,31	53		W	1	Hazy.
	2	0	64	65	30,24	64		SW	1	Fine.
31	7	0	59	63	30,28	56		W	1	Fine.
	2	0	72	66	30,25	72		W	1	Fine.

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1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
June 1	7	0	63	63	30,20	59	Rain this Month 1,212 Inches.	W	1	Fine.
	2	0	73	69	30,15	74		W byN	1	Fine.
2	7	0	62	65	30,20	57		E	1	Fine.
	2	0	73	69	30,19	73		E	1	Fine.
3	7	0	62	66	30,21	56		E	1	Fine.
	2	0	73	70	30,22	75		E	1	Fine.
4	7	0	62	66	30,20	57		E	1	Fine.
	2	0	77	71	30,16	77		E	1	Fine.
5	7	0	66	68	30,15	62		S	1	Hazy.
	2	0	78	71	30,15	70		E	1	Fine.
6	7	0	66	69	30,17	62		N	1	Fine.
	2	0	75	72	30,13	80		E	1	Fine.
7	7	0	64	66	30,29	60		N	1	Cloudy.
	2	0	74	70	30,13	78		E	1	Fine clear sky.
8	7	0	58	66	30.10	54		N	1	Cloudy.
	2	0	74	70	29,98	74		E	1	Fine.
9	7	0	65	68	29,92	60		E	1	Fine.
	2	0	77	72	29,93	79		E	1	Fine.
10	7	0	69	69	29,95	64		N	1	Fine. [at 9 P. M.
	2	0	81	74	29,94	84		SW	1	Fine; thunder and rain
11	7	0	70	71	30,10	65		N	1	Fine.
	2	0	78	73	30,11	78		N	1	Cloudy.
12	7	0	61	68	30,16	57		N	1	Fine.
	2	0	69	70	30,18	70		E	1	Fine.
13	7	0	54	62	30,20	50		E	1	Fine.
	2	0	70	70	30,08	70		E	1	Fine.
14	7	0	62	65	29,98	50		E	1	Fine.
	2	0	74	69	29,83	76		W	1	Cloudy.
15	7	0	67	67	29,65	63		ENE	1	Cloudy. [11 A. M.
	2	0	66	68	29,71	71		W	1	Fine; a thunder storm at

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1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
June 16	7	0	61	63	29,82	57	Rain this Month 1,212 Inches.	E	1	Cloudy.
	2	0	67	66	29,91	68		E	1	Cloudy.
17	7	0	58	63	30,16	54		E	1	Cloudy.
	2	0	65	65	30,19	66		NE	1	Cloudy.
18	7	0	59	62	30,22	54		SE	1	Fine.
	2	0	67	64	30,16	67		SE	1	Fine.
19	7	0	63	63	29,99	59		N by E	1	Fine, rather hazy.
	2	0	73	66	29,93	73		N	1	Cloudy.
20	7	0	61	63	29,93	59		N by E	1	Cloudy.
	2	0	65	63	30,02	70		NE	1	Cloudy.
21	7	0	55	63	30,19	51		NE	1	Fine.
	2	0	66	65	30,15	66		E	1	Fine.
22	7	0	59	63	30,13	53		E	1	Fine.
	2	0	73	68	30,06	73		E	1	Fine.
23	7	0	64	64	30,01	60		E	1	Cloudy; rain in the night.
	2	0	72	68	29,97	73		SSE	1	Fine.
24	7	0	66	65	30,01	63		W	1	Fine.
	2	0	73	67	30,00	75		W	1	Fine.
25	7	0	66	66	30,11	63		W	1	Fine.
	2	0	75	70	30,08	75		W	1	Fine.
26	7	0	67	67	30,07	62		W	1	Fine.
	2	0	76	71	29,97	77		SW	1	Fine.
27	7	0	65	68	30,06	62		N	1	Fine.
	2	0	73	70	30,15	77		N	1	Fine.
28	7	0	65	65	30,16	60		W	1	Cloudy.
	2	0	71	68	30,04	73		WSW	1	Cloudy.
29	7	0	60	65	30,00	59		NNE	1	Cloudy.
	2	0	70	65	30,05			NE	1	Cloudy.
30	7	0	60	64	30,03	56		SSW	1	Fine.
	2	0	65	65	29,91			W	1	Rain.

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1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
July 1	7	0	62	63	30,06	56	Rain this Month 2,788 Inches.	NE	1	Fine.
	2	0	69	66	30,07	69		W	1	Fine.
2	7	0	60	62	30,01	56		W	1	Fine.
	2	0	69	65	29,89	72		W	1	Cloudy.
3	7	0	59	63	29,93	56		W	1	Fine.
	2	0	71	68	29,95	71		W	1	Fine.
4	7	0	66	63	29,99	60		W	1	Fine.
	2	0	75	67	29,95	76		W	1	Cloudy. [lightning at 1 A.M.
5	7	0	65	65	29,79	63		E	1	Cloudy; a storm of thunder and
	2	0	69	66	29,71	73		W by N	1	Cloudy; a thunder storm at 11 A.M.
6	7	0	60	63	29,84	58		N	1	Cloudy.
	2	0	67	65	29,94	67		NNE	1	Cloudy.
7	7	0	60	62	30,09	55		NE	1	Fine.
	2	0	70	65	30,08	71		NW	1	Cloudy.
8	7	0	62	62	30,16	58		W	1	Cloudy.
	2	0	69	65	30,18	70		NW	1	Fine.
9	7	0	64	64	30,08	63		W	1	Cloudy.
	2	0	69	66	30,02	71		W	1	Rain.
10	7	0	65	63	29,87	62		SW	1,2	Cloudy.
	2	0	71	67	29,84	71		W	1	Fine.
11	7	0	65	64	29,86	61		W	1	Cloudy.
	2	0	69	65	29,73	72		S	1	Cloudy.
12	7	0	62	63	29,41	60		W	1	Fine.
	2	0	67	64	29,44	68		NNW	1	Fine.
13	7	0	58	62	29,81	54		N	1	Fine.
	2	0	68	64	29,93	69		W by N	1	Fine.
14	7	0	59	62	30,02	55		NE	1	Fine.
	2	0	69	65	30,00	70		N by E	1	Fine.
15	7	0	59	62	29,98	55		E	1	Cloudy and hazy.
	2	0	69	64	29,88	70		E	1	Cloudy.
16	7	0	61	63	29,78	58		N	1	Fine.
	2	0	69	64	29,74	70		N	1	Cloudy.

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1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
July 17	7	0	62	63	29,68	60	Rain this Month 2,788 Inches.	W	1	Rain.
	2	0	66	64	29,72	70		S	1	Cloudy.
18	7	0	65	63	29,74	60		E	1	Fine.
	2	0	73	68	29,70	73		E	1	Cloudy. [ning at 3 A.M.
19	7	0	64	65	29,50	64		S	1	Rain; thunder and light-
	2	0	72	67	29,53	72		S	1	Fine.
20	7	0	66	65	29,51	61		S	1	Cloudy.
	2	0	70	67	29,57	72		SW	2	Fine.
21	7	0	66	65	29,58	61		S	1	Fine.
	2	0	69	66	29,53	71		S	1	Cloudy.
22	7	0	65	62	29,55	63		S	1	Cloudy.
	2	0	71	66	29,73	71		NNW	1,2	Cloudy.
23	7	0	65	64	29,79	62		S	1	Cloudy.
	2	0	70	65	29,75	71		S	1,2	Cloudy.
24	7	0	67	65	29,59	62		SW	1	Cloudy.
	2	0	70	68	29,62	72		S	1	Fine.
25	7	0	65	65	29,66	61		SW	1,2	Rain.
	2	0	71	66	29,68	71		SW	2	Cloudy.
26	7	0	63	64	29,76	58		W	1	Fine.
	2	0	70	66	29,74	71		S	1	Cloudy.
27	7	0	62	64	29,80	58		SW	1	Fine clear sky.
	2	0	71	66	29,79	72		SW	1	Fine.
28	7	0	66	65	29,54	62		S	1	Fine.
	2	0	69	66	29,50	70		SSW	1	Showery.
29	7	0	64	63	29,52	64		NW	1	Cloudy.
	2	0	69	65	29,49	70		S	1	Cloudy.
30	7	0	62	63	29,54	59		NW	1	Fine.
	2	0	66	64	29,57	66		SW	1	Cloudy.
31	7	0	60	60	29,68	53		SW	1	Fine.
	2	0	62	62	29,68	67		W	2	Rain.

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1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Aug. 1	7	0	57	59	29,80	51	Rain this Month 0,975 Inches.	W by N	1	Fine.
	2	0	64	62	29,80	66		W	1	Fine.
2	7	0	58	60	29,72	56		W	1	Hazy.
	2	0	60	61	29,81	65		W	1	Fine; a hail storm at 1 P.M.
3	7	0	56	59	30,01	50		W	1	Fine.
	2	0	65	61	30,02	65		N by E	1	Cloudy.
4	7	0	58	61	29,89	55		N by E	1	Fine.
	2	0	67	62	29,81	67		NW	1	Cloudy.
5	7	0	59	61	29,86	53		N	1	Fine.
	2	0	68	62	29,88	66		N	1	Cloudy.
6	7	0	58	61	30,03	53		NW	1	Fine.
	2	0	65	62	30,02	68		N	1	Cloudy.
7	7	0	61	61	30,06	59		W	1	Cloudy.
	2	0	69	64	30,08	70		W	1	Cloudy.
8	7	0	59	61	30,03	57		E	1	Cloudy and hazy.
	2	0	71	63	29,91	72		E	1	Cloudy.
9	7	0	60	62	29,76	57		W	1	Fine.
	2	0	68	64	29,75	68		W	1	Cloudy.
10	7	0	63	63	29,80	62		W	1	Fine.
	2	0	70	64	29,81	70		W	1	Cloudy.
11	7	0	65	63	29,82	63		W	1	Cloudy.
	2	0	70	64	29,82	70		W	1	Cloudy.
12	7	0	64	64	29,84	62		W	1	Cloudy.
	2	0	70	65	29,84	70		W	1	Cloudy.
13	7	0	65	64	29,79	62		SW	1	Cloudy.
	2	0	71	66	29,84	71		NNW	1,2	Fine.
14	7	0	66	65	29,92	63		WSW	1	Fine.
	2	0	69	66	29,82	71		S	1,2	Cloudy.
15	7	0	63	65	29,69	63		N	1	Cloudy.
	2	0	68	66	29,86	70		W by N	1	Cloudy.
16	7	0	60	63	30,07	54		W	1	Cloudy.
	2	0	68	67	30,07	70		NW	1	Fine.

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1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Aug. 17	7	0	61	64	30,23	59	Rain this Month 0,975 Inches.	W	1	Fine.
	2	0	73	71	30,20	76		W	1	Fine.
18	7	0	62	66	30,19	68		W	1	Cloudy.
	2	0	73	71	30,15	75		W	1	Fine.
19	7	0	63	67	30,13	67		NE	1	Cloudy.
	2	0	74	71	30,15	75		SE	1	Fine.
20	7	0	62	67	30,14	66		ESE	1	Fine.
	2	0	70	71	30,14	71		E	1	Fine.
21	7	0	64	67	30,02	62		N	1	Hazy.
	2	0	76	73	29,97	78		E	1	Fine.
22	7	0	70	70	29,89	68		W	1	Cloudy.
	2	0	78	73	29,87	79		W	1	Fine, rather cloudy.
23	7	0	65	70	29,97	63		NE	1	Cloudy.
	2	0	78	72	29,98	80		WNW	1	Cloudy.
24	7	0	59	66	29,82	59		SW	1	Cloudy.
	2	0	67	68	29,79	70		S	1	Cloudy.
25	7	0	60	66	29,74	55		W	1	Fine, though somewhat cloudy.
	2	0	67	65	29,74	68		W	1,2	Cloudy.
26	7	0	59	64	29,68	56		W	1	Fine.
	2	0	66	65	29,65	67		WbyN	1	Fine.
27	7	0	60	64	29,69	56		W	1	Fine.
	2	0	62	64	29,70	66		WbyN	1	Showery.
28	7	0	60	64	29,72	57		W	1	Cloudy.
	2	0	66	64	29,63	66		ESE	2,1	Cloudy and showery.
29	7	0	60	63	29,47	58		W	1	Fine.
	2	0	65	64	29,53	67		W	2	Cloudy.
30	7	0	57	62	29,77	54		W	1	Fine.
	2	0	66	62	29,80	66		SW	1	Cloudy.
31	7	0	57	61	29,91	52		W	1	Fine.
	2	0	65	65	29,94	66		NW	1	Fine.

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1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Sept. 1	7	0	55	62	30,11	53	Rain this Month 0,702 Inches.	W by S	1	Cloudy.
	2	0	65	64	30,11	65		W	1	Cloudy.
2	7	0	58	60	30,08	54		SW	1	Fine.
	2	0	65	62	30,03	68		W	2	Cloudy.
3	7	0	62	62	29,94	61		W	1	Fine.
	2	0	68	64	29,91	69		W	1	Fine.
4	7	0	58	61	30,04	56		W	1	Fine.
	2	0	68	72	30,04	69		W	1	Fine.
5	7	0	63	64	29,93	61		W	1	Fine.
	2	0	68	65	29,85	70		WSW	1	Cloudy.
6	7	0	65	64	29,83	63		SW	1	Cloudy.
	2	0	68	69	29,83	70		WSW	1	Fine.
7	7	0	56	62	30,04	54		W	1	Fine.
	2	0	65	66	30,07	68		W by N	1	Fine.
8	7	0	58	62	29,92	55		W	1	Cloudy.
	2	0	66	64	29,89	68		W	1	Rain.
9	7	0	56	60	29,98	54		NW	1	Cloudy.
	2	0	64	62	29,96	66		WSW	2	Cloudy.
10	7	0	52	61	30,12	51		W	1	Fine.
	2	0	59	62	30,08	64		W	1	Fine.
11	7	0	58	63	29,92	51		S	1,2	Fine.
	2	0	64	64	29,94	67		W	1	Cloudy.
12	7	0	54	60	29,95	53		NW	1	Fine, rather hazy.
	2	0	62	61	30,01	65		N by E	1	Cloudy.
13	7	0	56	59	29,97	54		N	1	Cloudy.
	2	0	60	63	30,08	61		N	1	Cloudy.
14	7	0	50	59	29,91	45		E	1	Fine.
	2	0	61	61	30,16	62		E	1,2	Cloudy.
15	7	0	56	60	30,08	53		E	1	Cloudy and hazy.
	2	0	60	61	30,01	61		E	1	Cloudy.

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1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Sept. 16	7	0	54	60	30,04	52	Rain this Month 0,702 Inches.	W	1	Fine.
	2	0	66	64	30,03	66		W	1	Fine.
17	7	0	57	61	30,06	55		W	1	Cloudy and hazy.
	2	0	70	63	30,05	70		E	1	Fine.
18	7	0	55	61	30,08	52		N by E	1	Cloudy and hazy.
	2	0	63	69	30,09	70		S by E	1	Fine.
19	7	0	57	62	30,14	53		E	1	Fine.
	2	0	63	68	30,12	65		E	1,2	Fine.
20	7	0	55	60	29,99	52		N	1	Fine.
	2	0	60	71	29,93	64		E	1	Fine.
21	7	0	52	60	29,83	53		E	1	Fine.
	2	0	59	68	29,84	62		E	1	Fine.
22	7	0	52	59	29,88	59		E	1	Cloudy.
	2	0	61	60	29,86	62		E	1	Cloudy.
23	7	0	55	56	29,82	55		E	1	Rain.
	2	0	58	59	29,65	62		ENE	1	Rain.
24	7	0	57	57	29,44	56		E	1	Rain.
	2	0	60	60	29,31	60		E	1	Cloudy and showery.
25	7	0	52	57	29,31	51		NE	1	Fine.
	2	0	57	58	29,45	58		NE	1	Cloudy.
26	7	0	49	68	29,67	47		N	1	Cloudy and hazy.
	2	0	57	61	29,84	59		NE	1	Fine.
27	7	0	49	55	30,07	48		NE	1,2	Fine.
	2	0	56	57	30,16	57		SE	2	Cloudy.
28	7	0	51	56	30,21	49		N	1	Rain.
	2	0	56	57	30,17	57		N	1	Cloudy.
29	7	0	51	56	30,03	49		NE	1	Fine.
	2	0	58	57	29,96	59		E	1	Fine.
30	7	0	49	56	29,86	46		W	1	Cloudy and hazy.
	2	0	59	62	29,79	59		SE	1	Cloudy.

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1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Oct. 1	8	0	48	57	29,71	46	Rain this Month 3,053 Inches.	N	1	Hazy.
	2	0	59	60	29,68	59		SE	1	Fine.
2	8	0	57	59	29,74	55		E	1	Cloudy.
	2	0	63	60	29,75	64		E	1	Cloudy.
3	8	0	60	59	29,70	57		E	1	Rain.
	2	0	65	62	29,68	65		E	1	Cloudy.
4	8	0	59	61	29,78	58		S	1	Cloudy.
	2	0	64	65	29,71	66		W	1	Fine; much lightning this evening.
5	8	0	60	62	29,70	58		SW	1	Fine.
	2	0	63	62	29,68			SW	1	Fair.
6	8	0	54	61	29,53	52		W	1	Fine.
	2	0	59	60	29,57	60		W	1	Cloudy.
7	8	0	55	59	29,43			W	2	Fine.
	2	0	59	63	29,61					Cloudy.
8	8	0	58	60	29,56	55		W	3	Rain.
	2	0	59	63	29,62	60		W	1,2	Fine.
9	8	0	57	60	29,71	53		S	2	Cloudy.
	2	0	52	60	29,82	50		W	1	Fine.
10	8	0	52	60	29,82	50		W	1	Fine.
	2	0	64	64	29,91	60		NW	1	Fine.
11	8	0	48	59	30,14	46		W	1	Fine.
	2	0	58	63	30,15	59		SW	1	Fine.
12	8	0	54	60	29,97	52		SE	1	Cloudy.
	2	0	60	62	29,81	61		SE	1	Cloudy.
13	8	0	58	61	29,45	59		S	2	Cloudy.
	2	0	61	61	29,38	62		SE	1,2	Rain.
14	8	0	46	59	29,83	46		N	2	Fine.
	2	0	52	61	29,98	52		N	1,2	Cloudy.
15	8	0	43	57	29,93	41		W	1	Fine.
	2	0	50	59	29,74	51			1	Rain.
16	8	0	50	58	29,37	48		E	1	Cloudy and hazy.
	2	0	53	60	29,32	54		W	1	Rain.

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for October, 1822.

1822	Time.	Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H. M.	°	°	Inches.			Points.	Str.	
Oct. 17	8 0	51	57	29,31	50	Rain this Month 3,053 Inches.	N by E	2	Rain.
	2 0	51	58	29,34	53		N	2	Cloudy and Rain
18	8 0	41	56	29,61	41		NW	1	Fine; rather hazy.
	2 0	50	59	29,66	50		W	1	Fine.
19	8 0	48	56	29,49	46		S	1,2	Cloudy.
	2 0	53	59	29,47	55		W	1,2	Fine.
20	8 0	54	58	29,55	52		S	1,2	Cloudy.
	2 0	59	58	29,41	59		SW	3	Rain.
21	8 0	53	57	29,54	51		S	1,2	Fine.
	2 0	59	59	29,61			S	1	Fine.
22	8 0	48	58	29,72	47		W	1	Fine.
	2 0	59	60	29,81	59		SW	1	Fine.
23	8 0	46	56	29,70	44		E	1	Fine, rather hazy.
	2 0	55	58	29,57	56		E	1	Cloudy.
24	8 0	57	57	29,38	55		SSE	1	Cloudy.
	2 0	61	59	29,36	62		E	1	Cloudy.
25	8 0	55	58	29,48	55		S	1	Fine.
	2 0	60	60	29,51	62		S by E	1	Fine.
26	8 0	53	58	29,53	52		E	1	Fine.
	2 0	58	59	29,43	60		SE	1	Cloudy.
27	8 0	52	58	29,53	51		NW	1	Cloudy.
	2 0	55	59	29,58	57		W	1	Fine.
28	8 0	44	47	29,77	43		E	1	Hazy and cloudy.
	2 0	54	58	29,84	55		SSW	1	Fine.
29	8 0	53	56	29,88	49		S	1	Rain.
	2 0	59	57	29,96	59		N	1	Cloudy.
30	8 0	55	56	29,97	53		E	1	
	2 0	60	58	29,93	61		SW	1	Cloudy.
31	8 2	54	56	29,69	52		E	1	Fine.
	2 2	60	60	29,71	61		S	1	Fine.

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for November, 1822.

1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Nov. 1	8	0	56	58	29,86	53	Rain this Month 3,607 Inches.	S	2	Cloudy.
	2	0	60	58	29,83	61		S	2	Cloudy. [the night.
2	8	0	60	59	29,69	58		SE	1	Fine; rain and wind in
	2	0	61	62	29,70	61		S	1	Cloudy.
3	8	0	54	56	29,97	53		SW	1	Fine.
	2	0	58	57	30,04	61		SW	1	Cloudy.
4	8	0	48	54	30,26	47		W	1	Hazy.
	2	0	52	57	30,26	58		W	1	Fine.
5	8	0	52	56	30,27	47		W	1	Cloudy.
	2	0	56	58	30,20	57		SW	1	Cloudy.
6	8	0	52	57	30,18	51		W	1	Cloudy.
	2	0	56	60	30,13	57		W	1	Cloudy.
7	8	0	50	57	29,95	49		S	1	Fine.
	2	0	54	59	29,95			S	1	Fine.
8	8	0	49	58	29,93	49		W	1	Cloudy.
	2	0	57	61	29,93	58		N by E	1	Cloudy and dark.
9	8	0	43	58	29,94	43			1	Thick fog.
	2	0	49	57	30,01	56		N	1	Cloudy.
10	8	0	50	57	29,70	48		N	1	Rain.
	2	0	53	58	29,88	53		N by E	1	Cloudy.
11	8	0	46	57	30,24	46			1	Thick fog.
	2	0	57	60	30,24	58		S	1	Fine.
12	8	0	52	58	30,12	49		SSE	2	Cloudy.
	2	0	55	59	30,02	56		S	1,2	Cloudy.
13	8	0	48	58	29,92	47		NW	1	Foggy.
	2	0	50	60	29,75	51		W	1	Fine; streaky clouds.
14	8	0	46	55	29,40	45		W	1	Fine.
	2	0	51	59	29,51	52		W	1	Fine.
15	8	0	48	57	29,24	45		SW	2	Cloudy.
	2	0	47	59	29,29	49		W	1	Fine.
16	8	0	42	56	29,29	42		N	1	Rain.
	2	0	42	58	29,29	48		NNW	1	Rain.

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1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Nov. 17	8	0	41	55	29,61	39	Rain this Month 3,607 Inches.	WSW	1	Fine.
	2	0	48	56	29,62	48		WSW	1	Cloudy.
18	8	0	51	54	29,62	47		S	2,3	Rain.
	2	0	52	58	29,68	55		SW	1	Fine.
19	8	0	52	56	29,86	50		S	1,2	Cloudy.
	2	0	54	58	29,78	55		SW	2	Cloudy.
20	8	0	53	57	29,63	53		E	1	Rain and fog.
	2	0	54	59	29,62	56		S	1	Rain.
21	8	0	52	58	29,60	51		W	1,2	Cloudy.
	2	0	53	61	29,72	55		W	1	Fine.
22	8	0	47	57	29,88	45		S	1	Cloudy.
	2	0	51	60	29,72	52		S	2,3	Rain.
23	8	0	51	58	29,61	51		W	2	Rainy and squally.
	2	0	51	59	29,64	53		SW	1	Fine.
24	8	0	47	57	29,88	46		W	1	Fine.
	2	0	53	57	29,79	53		S	1	Cloudy.
25	8	0	51	56	29,55	51		S	2	Rain.
	2	0	52	59	29,50	53		SSW	1,2	Fine.
26	8	0	48	56	29,53	48		W	1	Fine.
	2	0	52	60	29,55	53		S	2	Cloudy.
27	8	0	51	57	29,57	50		WSW	1	Fine.
	2	0	53	59	29,65	56		W	1	Fine.
28	8	0	45	56	29,52	44		S by E	2	Rain.
	2	0	48	60	29,31	50		S	1	Cloudy.
29	8	0	41	56	29,47	41		W	1	Fine.
	2	0	48	58	29,30	48		S	1,2	Cloudy.
30	8	0	38	54	29,36	38		SW	1	Fine.
	2	0	45	55	29,37	45		W	1	Rain.

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for December, 1822.

1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	o	o	Inches.			Points.	Str.	
Dec. 1	8	0	42	53	29,31	40	Rain this Month 1,041 Inches.	S	1	Fine.
	2	0	54	54	29,16	54		SSW	2	Rainy and squally.
2	8	0	40	52	29,07	40		SW	1	Fine.
	2	0	48	55	29,04	50		SSW	1	Fine.
3	8	0	41	52	29,23	41		NNW	1	Cloudy and foggy.
	2	0	42	55	29,41	43		NNW	1	Fine.
4	8	0	39	51	29,59	38		SW	1	Fine.
	2	0	48	54	29,42	48		SW	1,2	Rain.
5	8	0	37	51	29,75	36		W	1	Fine.
	2	0	42	54	29,67	45		S	1,2	Cloudy.
6	8	0	43	52	29,50			W	1	Fine.
	2	0	45	56	29,64	47		W by N	1	Fine.
7	8	0	39	52	29,94	38		W	1	Fine.
	2	0	43	55	29,98	43		NW	1	Fine.
8	8	0	38	51	30,22	37		W	1	Fine.
	2	0	41	52	30,18	43		SW	1	Fine.
9	8	0	46	51	30,13	42		S	2	Cloudy.
	2	0	47	54	30,09	48		SW	2	Rain.
10	8	0	41	52	30,33	41		N	1	Fine, rather hazy.
	2	0	44	54	30,42	47		NE	1	Fine.
11	8	0	37	51	30,54	35		N	1	Foggy.
	2	0	37	53	30,56	42		N	1	Foggy.
12	8	0	37	51	30,57	37		NNE	1	Foggy.
	2	0	42	53	30,55	43		E	1	Fine.
13	8	0	38	52	30,40	37		E	1	Fine.
	2	0	42	53	30,32	43		E	1	Hazy.
14	8	0	37	51	30,26	37		E	1	Cloudy.
	2	0	40	53	30,24	41		SW	1	Cloudy.
15	8	0	36	50	30,08	35		E	1	Cloudy.
	2	0	38	49	30,03	40		E	1	Cloudy.
16	8	0	35	48	30,18	35		NNE	1	Cloudy and hazy.
	2	0	34	50	30,22	35		NE	1	Cloudy and hazy.

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for December, 1822.

1822	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Dec. 17	8	0	34	47	30,33	34	Rain this Month 1,041 Inches.	NNW	1	Cloudy and hazy.
	2	0	36	50	30,31	37		W	1	Cloudy and hazy.
18	8	0	44	47	30,23	37		N	1	Cloudy and hazy.
	2	0	46	49	30,22	47		N by E	1	Rain.
19	8	0	39	47	30,27	37		NW	1	Fine.
	2	0	38	49	30,28	40		NE	1	Cloudy.
20	8	0	31	48	30,35	31		N by E	1	Fine, but rather hazy.
	2	0	34	50	30,36	36		E	1	Fine.
21	8	0	31	46	30,34	29		N	1	Fine.
	2	0	35	49	30,29	36		N	1	Fine.
22	8	0	34	45	30,14	30		N	1	Fine.
	2	0	39	46	30,09	39		NE	1	Cloudy.
23	8	0	38	44	30,07	35		N	1	Hazy.
	2	0	42	48	30,03	43		NE	1	Cloudy.
24	8	0	38	46	30,05	37		E	1	Foggy.
	2	0	40	48	30,09	42		E	1	Cloudy.
25	8	0	36	45	30,42	34		E	1	Cloudy and hazy.
	2	0	36	45	30,43	37		E	1	Cloudy and hazy.
26	8	0	31	44	30,45	31		E	1	Hazy.
	2	0	35	48	30,38	36		SE	1	Fine.
27	8	0	32	44	30,37	30		E	1	Cloudy and hazy.
	2	0	44	44	30,32	44		ESE	1	Cloudy.
28	8	0	28	42	30,27	27		E	1	Hazy.
	2	0	32	46	30,20	34		E by S	1	Fine.
29	8	0	30	41	30,15	27		E	1	Cloudy.
	2	0	31	42	30,08	35		E	1	Fine.
30	8	0	25	39	29,83	23		E	1	Fine.
	2	0	31	43	29,80	29		SE	1,2	Fine.
31	8	0	30	40	29,74	28		E	1	Cloudy and dark.
	2	0	32	45	29,71	32		E	1	Cloudy.

1822.	Thermometer without.			Thermometer within.			Barometer.*			Six's Thermometer.			Rain.†
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	50	32	41,5	58	41	50,3	30,17	29,10	30,107	51	31	42,2	0,215
February	53	35	45,6	60	49	55,0	30,69	29,30	30,102	54	34	45,5	0,560
March	65	36	49,5	63	51	56,7	30,36	29,43	30,030	66	35	49,5	1,273
April	63	37	49,6	63	48	56,3	30,40	29,30	29,921	64	35	49,5	1,620
May	74	47	59,4	70	54	61,6	30,46	29,30	29,959	75	45	58,8	1,022
June	81	54	67,0	74	62	67,0	30,29	29,65	30,064	84	50	64,6	1,212
July	75	58	66,1	68	60	64,4	30,18	29,41	29,775	76	53	64,9	2,788
August	78	56	64,8	73	59	64,7	30,23	29,47	29,992	80	50	64,3	0,975
September	70	49	58,6	72	55	61,6	30,21	29,31	29,941	70	45	58,6	0,702
October	65	41	55,1	65	47	59,0	30,15	29,31	29,663	66	41	54,3	3,053
November	61	38	50,6	62	54	57,6	30,27	29,24	29,748	61	38	49,7	3,607
December	54	25	38,1	56	39	49,0	30,57	29,04	30,059	54	23	58,0	1,041
Whole year			53,8			58,6			29,863			55,0	18,068

* The quicksilver in the bason of the barometer is 81 feet above the level of low water spring tides at Somerset-house.

† The Society's Rain Gage is 114 feet above the same level, and 75 feet 6 inches above the surrounding ground.

The mean variation of the Magnetic Needle will be given in Part II.

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXIII.

PART II.

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MDCCCXXIII.

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Mean Variation of the Magnetic Needle.

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Corrections.

Part I., p. 145, l. 9, and p. 148, l. 12, for *decipimenta*, read *dissepimenta*.

PHILOSOPHICAL

TRANSACTIONS.

XIII. *On a new phenomenon of electro-magnetism. By Sir* HUMPHRY DAVY, *Bart. Pres. R. S.*

Read March 6, 1823.

ON a subject so obscure as electro-magnetism, and connected by analogies more or less distinct with the doctrines of heat, light, electricity, and chemical attraction, it is not difficult to frame *hypotheses;* but the science is in a state too near its infancy to expect the developement of any satisfactory *theory;* and its progress can only be ensured by new facts and experiments, which may prepare the way for extensive and general reasonings upon its principles. Influenced by this opinion, I am induced to lay before the Society an account of an electro-magnetic phenomenon I observed about fifteen months ago in the laboratory of the Royal Institution, and which I have lately had an occasion of witnessing in a more perfect manner, through the kindness of Mr. PEPYS, by the use of a large battery, constructed under his directions for the London Institution, and containing a pair of

plates of about two hundred square feet. In describing this phenomenon, I shall not enter into very minute details, because the experiments, which led to the discovery of it, are very simple, and, though more distinct with a large apparatus, yet it may be observed by the use of a pair of plates containing from ten to fifteen square feet.

Immediately after Mr. FARADAY had published his ingenious experiments on electro-magnetic rotation, I was induced to try the action of a magnet on mercury connected in the electrical circuit, hoping that, in this case, as there was no mechanical suspension of the conductor, the appearances would be exhibited in their most simple form; and I found that when two wires were placed in a basin of mercury perpendicular to the surface, and in the voltaic circuit of a battery with large plates; and the pole of a powerful magnet held either above or below the wires, the mercury immediately began to revolve round the wire as an axis, according to the common circumstances of electro-magnetic rotation, and with a velocity exceedingly increased when the *opposite* poles of two magnets were used, one above, the other below.

Masses of mercury of several inches in diameter were set in motion, and made to revolve in this manner, whenever the pole of the magnet was held near the perpendicular of the wire; but when the pole was held above the mercury between the two wires, the circular motion ceased; and currents took place in the mercury in opposite directions, one to the right, and the other to the left of the magnet. These circumstances, and various others which it would be tedious to detail, induced me to believe that the passage of the electricity through the mercury produced motions independent of

the action of the magnet; and that the appearances which I have described were owing to a composition of forces.

I endeavoured to ascertain the existence of these motions in the mercury, by covering its surface with weak acids; and diffusing over it finely divided matter, such as the seeds of lycopodium, white oxide of mercury, &c. but without any distinct result. It then occurred to me, that from the position of the wires, currents, if they existed, must occur chiefly in the lower, and not the upper surface of the mercury: and I consequently inverted the form of the experiment. I had two copper wires, of about one-sixth of an inch in diameter, the extremities of which were flat and carefully polished, passed through two holes three inches apart in the bottom of a glass basin, and perpendicular to it; they were cemented into the basin, and made non-conductors by sealing-wax, except at their polished ends; the basin was then filled with mercury, which stood about a tenth or twelfth of an inch above the wires. The wires were now placed in a powerful voltaic circuit. The moment the contacts were made, *the phenomenon*, which is the principal object of this paper, occurred: the mercury was immediately seen in violent agitation; its surface became elevated into a small cone above each of the wires; waves flowed off in all directions from these cones; and the only point of rest was apparently where they met in the centre of the mercury between the two wires. On holding the pole of a powerful bar magnet at a considerable distance (some inches) above one of the cones, its apex was diminished and its base extended: by lowering the pole further, these effects were still further increased, and the undulations were feebler. At a smaller distance the surface of the mercury

became plane; and rotation slowly began round the wire. As the magnet approached, the rotation became more rapid, and when it was about half an inch above the mercury, a great depression of it was observed above the wire, and a vortex, which reached almost to the surface of the wire.

In the first experiments which I made, the conical elevations or fountains of mercury were about the tenth or twelfth of an inch high, and the vortices apparently as low; but in the experiments made at the London Institution, the mercury being much higher above the wire, the elevations and depressions were much more considerable, amounting to the fifth or sixth of an inch. Of course, the rotation took place with either pole of a magnet or either wire, or both together, according to the well known circumstances which determine these effects.

To ascertain whether the communication of heat diminishing the specific gravity of the mercury, had any share in these phenomena, I placed a delicate thermometer above one of the wires in the mercury, but there was no immediate elevation of temperature; the heat of the mercury gradually increased, as did that of the wires; but this increase was similar in every part of the circuit. I proved the same thing more distinctly, by making the whole apparatus a *thermometer* terminating in a fine tube filled with mercury. At the first instant that the mercury became electro-magnetic, there was no increase of its volume.

This phenomenon cannot be attributed to common electrical repulsion; for in the electro-magnetic circuit, similar electrified conductors do not repel, but attract each other; and it is in the case in which conductors in *opposite* states are

brought near each other on surfaces of mercury, that repulsion takes place.

Nor can the effect be referred to that kind of action which occurs when electricity passes from good into bad conductors, as in the phenomena of points electrified in air, as the following facts seem to prove. Steel wires were substituted for copper wires, and the appearances were the same in kind, and only less in degree; without doubt, in consequence of a smaller quantity of electricity passing through the steel wires: and by comparing the conducting powers of equal cylinders of mercury and steel in glass tubes, by ascertaining the quantity of iron filings they attracted, it was found that the conducting powers of mercury were higher than those of steel; the first metal taking up fifty-eight grains of iron filings, and the second only thirty-seven.

Again; fused tin was substituted for mercury in a porcelain vessel into which wires of copper and steel were alternately ground and fixed: the elevations were produced as in the mercury, and the phenomena of rotation by the magnet; and it was found by direct experiment, that the conducting powers of the tin, at and just before its point of fusion, did not perceptibly differ, and that they were much higher than those of mercury. Lastly, the communication was made from the battery by two tubes having nearly the same diameter as the wires, filled with mercury, so that the electricity, for some inches before it entered the basin, passed through mercury; and still the appearances continued the same.

From the rapidity of the undulations round the points of the cones, I thought they would put in motion any light bodies placed above the mercury; but I could not produce

the slightest motion in a very light wheel hung on an axle; and when fine powders of any kind were strewed upon the surface, they merely underwent undulations, without any other change of place; and fine iron filings strewed on the top of the cone, arranged themselves in right lines at right angles to the line joining the two wires, and remained stationary, even on the centre of the cone. The effect, therefore, is of a novel kind, and in one respect seems analogous to that of the tides. It would appear as if the passage of the electricity diminished the action of gravity on the mercury. And that there is no change of volume of the whole mass of the mercury appears from the experiment, page 156; and this was shown likewise by enclosing the apparatus in a kind of manometer, terminating in a fine tube containing air enclosed by oil; and which, by its expansion or contraction, would have shown the slightest change of volume in the mercury: none however took place when the contacts were alternately made and broken, unless the circuit was uninterrupted for a sufficient time to communicate sensible heat to the mercury.

This phenomenon, in which the same effects are produced at the two opposite poles, seems strongly opposed to the idea of the electro-magnetic results being produced by the transition currents or motions of a single imponderable fluid.

On the conjectural part of the subject I shall not however enter, for the reasons stated in the beginning of this paper; but I cannot with propriety conclude, without mentioning a circumstance in the history of the progress of electro-magnetism, which, though well known to many Fellows of this Society, has, I believe, never been made public, namely, that we owe to the sagacity of Dr. WOLLASTON, the first idea of

the possibility of the rotations of the electro-magnetic wire round its axis, by the approach of a magnet; and I witnessed, early in 1821, an unsuccessful experiment which he made to produce the effect in the laboratory of the Royal Institution.

XIV. *On fluid Chlorine. By M.* Faraday, *Chemical Assistant in the Royal Institution. Communicated by Sir* H. Davy, *Bart. Pres. R. S.*

Read March 13, 1823.

It is well known that before the year 1810, the solid substance obtained by exposing chlorine, as usually procured, to a low temperature, was considered as the gas itself reduced into that form; and that Sir Humphry Davy first showed it to be a hydrate, the pure dry gas not being condensible even at a temperature of—40° F.

I took advantage of the late cold weather to procure crystals of this substance for the purpose of analysis. The results are contained in a short paper in the Quarterly Journal of Science, Vol. XV. Its composition is very nearly 27.7 chlorine, 72.3 water, or 1 proportional of chlorine, and 10 of water.

The President of the Royal Society having honoured me by looking at these conclusions, suggested, that an exposure of the substance to heat under pressure, would probably lead to interesting results; the following experiments were commenced at his request. Some hydrate of chlorine was prepared, and being dried as well as could be by pressure in bibulous paper, was introduced into a sealed glass tube, the upper end of which was then hermetically closed. Being placed in water at 60°, it underwent no change; but when put into water at 100°, the substance fused, the tube became

filled with a bright yellow atmosphere, and, on examination, was found to contain two fluid substances: the one, about three-fourths of the whole, was of a faint yellow colour, having very much the appearance of water; the remaining fourth was a heavy bright yellow fluid, lying at the bottom of the former, without any apparent tendency to mix with it. As the tube cooled, the yellow atmosphere condensed into more of the yellow fluid, which floated in a film on the pale fluid, looking very like chloride of nitrogen; and at 70° the pale portion congealed, although even at 32° the yellow portion did not solidify. Heated up to 100° the yellow fluid appeared to boil, and again produced the bright coloured atmosphere.

By putting the hydrate into a bent tube, afterwards hermetically sealed, I found it easy, after decomposing it by a heat of 100°, to distil the yellow fluid to one end of the tube, and so separate it from the remaining portion. In this way a more complete decomposition of the hydrate was effected, and, when the whole was allowed to cool, neither of the fluids solidified at temperatures above 34°, and the yellow portion not even at 0°. When the two were mixed together they gradually combined at temperatures below 60°, and formed the same solid substances as that first introduced. If, when the fluids were separated, the tube was cut in the middle, the parts flew asunder as if with an explosion, the whole of the yellow portion disappeared, and there was a powerful atmosphere of chlorine produced; the pale portion on the contrary remained, and when examined, proved to be a weak solution of chlorine in water, with a little muriatic acid, probably from the impurity of the hydrate used. When that end of the tube in which the yellow fluid lay was broken under

a jar of water, there was an immediate production of chlorine gas.

I at first thought that muriatic acid and euchlorine had been formed; then, that two new hydrates of chlorine had been produced; but at last I suspected that the chlorine had been entirely separated from the water by the heat, and condensed into a dry fluid by the mere pressure of its own abundant vapour. If that were true, it followed, that chlorine gas, when compressed, should be condensed into the same fluid, and, as the atmosphere in the tube in which the fluid lay was not very yellow at 50° or 60°, it seemed probable that the pressure required was not beyond what could readily be obtained by a condensing syringe. A long tube was therefore furnished with a cap and stop-cock, then exhausted of air and filled with chlorine, and being held vertically with the syringe upwards, air was forced in, which thrust the chlorine to the bottom of the tube, and gave a pressure of about 4 atmospheres. Being now cooled, there was an immediate deposit in films, which appeared to be hydrate, formed by water contained in the gas and vessels, but some of the yellow fluid was also produced. As this however might also contain a portion of the water present, a perfectly dry tube and apparatus were taken, and the chlorine left for some time over a bath of sulphuric acid before it was introduced. Upon throwing in air and giving pressure, there was now no solid film formed, but the clear yellow fluid was deposited, and more abundantly still upon cooling. After remaining some time it disappeared, having gradually mixed with the atmosphere above it, but every repetition of the experiment produced the same results.

Presuming that I had now a right to consider the yellow fluid as pure chlorine in the liquid state, I proceeded to examine its properties, as well as I could when obtained by heat from the hydrate. However obtained, it always appears very limpid and fluid, and excessively volatile at common pressure. A portion was cooled in its tube to 0°: it remained fluid. The tube was then opened, when a part immediately flew off, leaving the rest so cooled by the evaporation as to remain a fluid under the atmospheric pressure. The temperature could not have been higher than —40° in this case; as Sir HUMPHRY DAVY has shown that dry chlorine does not condense at that temperature under common pressure. Another tube was opened at a temperature of 50°; a part of the chlorine volatilised, and cooled the tube so much as to condense the atmospheric vapour on it as ice.

A tube having the water at one end and the chlorine at the other was weighed, and then cut in two; the chlorine immediately flew off, and the loss being ascertained was found to be 1.6 grains: the water left was examined and found to contain some chlorine: its weight was ascertained to be 5.4 grains. These proportions, however, must not be considered as indicative of the true composition of hydrate of chlorine; for, from the mildness of the weather during the time when these experiments were made, it was impossible to collect the crystals of hydrate, press, and transfer them, without losing much chlorine; and it is also impossible to separate the chlorine and water in the tube perfectly, or keep them separate, as the atmosphere within will combine with the water, and gradually reform the hydrate.

Before cutting the tube, another tube had been prepared

exactly like it in form and size, and a portion of water introduced into it, as near as the eye could judge, of the same bulk as the fluid chlorine: this water was found to weigh 1.2 grains; a result, which, if it may be trusted, would give the specific gravity of fluid chlorine as 1.33; and from its appearance in, and on water, this cannot be far wrong.

Note on the condensation of Muriatic Acid Gas into the liquid form. By Sir H. DAVY, *Bart. Pres. R. S.*

IN desiring Mr. FARADAY to expose the hydrate of chlorine to heat in a closed glass tube, it occurred to me, that one of three things would happen; that it would become fluid as a hydrate; or that a decomposition of water would occur, and euchlorine and muriatic acid be formed; or that the chlorine would separate in a condensed state. This last result having been obtained, it evidently led to other researches of the same kind. I shall hope, on a future occasion, to detail some general views on the subject of these researches. I shall now merely mention, that by sealing muriate of ammonia and sulphuric acid in a strong glass tube, and causing them to act upon each other, I have procured liquid muriatic acid: and by substituting carbonate for muriate of ammonia, I have no doubt that carbonic acid may be obtained, though in the only trial I have made the tube burst. I have requested Mr. FARADAY to pursue these experiments, and to extend them to all the gases which are of considerable density, or to any extent soluble in water; and I hope soon to be able to lay an account of his results, with

some applications of them that I propose to make, before the Society.

I cannot conclude this note without observing, that the generation of elastic substances in close vessels, either with or without heat, offers much more powerful means of approximating their molecules than those dependent upon the application of cold, whether natural or artificial: for, as gases diminish only about $\frac{1}{480}$ in volume for every — degree of FAHRENHEIT'S scale, beginning at ordinary temperatures, a very slight condensation only can be produced by the most powerful freezing mixtures, not half as much as would result from the application of a strong flame to one part of a glass tube, the other part being of ordinary temperature: and when attempts are made to condense gases into fluids by sudden mechanical compression, the heat, instantly generated, presents a formidable obstacle to the success of the experiment; whereas, in the compression resulting from their slow generation in close vessels, if the process be conducted with common precautions, there is no source of difficulty or danger; and it may be easily assisted by artificial cold in cases when gases approach near to that point of compression and temperature at which they become vapours.

XV. *On the motions of the Eye, in illustration of the uses of the muscles and nerves of the orbit.* *By* Charles Bell, *Esq. Communicated by Sir* Humphry Davy, *Bart. P. R. S.*

Read March 20, 1823.

The object of this paper is to explain the reason of there being six nerves distributed to the eye, and consequently crowded into the narrow space of the orbit.

But before it is possible to assign the uses of these nerves, we must examine the motions of the eye more minutely than has hitherto been done, and try to comprehend the offices to be performed. Much as the eye has been studied, the frame-work which suspends it, and by which it is moved and protected, has not received the attention it deserves. Yet this frame-work, or apparatus, is not less calculated to renew our wonder, than the properties of the organ itself.

It is therefore necessary to divide the paper into two parts. *First*, to show the uses of the apparatus which is exterior to the eye-ball; and then, in the *second place*, to consider how the nerves minister to these offices.

Part I.

Of the muscles and frame-work which are around the eye-ball.

Even grave and learned men have eulogized this organ as the most necessary to intellectual enjoyment, and which ranges from the observation of the fixed stars, to that of the expres-

sion in the human face. But this admiration is in part misplaced, if given to the optic nerve and ball of the eye exclusively; since these high endowments belong to the exercise of the whole eye, its exterior apparatus as much as to that nerve which is sensible to the impressions of light. It is to the muscular apparatus, and to the conclusions we are enabled to draw from the consciousness of muscular effort, that we owe that geometrical sense, by which we become acquainted with the form, and magnitude, and distance of objects. We might as well expect to understand the uses of a theodolite, or any complicated instrument for observations, by estimating the optical powers of the glasses, without considering the quadrant, level, or plumb-line, as expect to learn the whole powers of the eye by confining our study to the naked ball. I propose to show, that we must distinguish the motions of the eye, according to their objects or uses, whether for the direct purpose of vision, or for the preservation of the organ: that the eye undergoes a revolving motion not hitherto noticed; that it is subject to a state of rest and activity, and that the different conditions of the retina are accompanied by appropriate conditions of the surrounding muscles; that these muscles are to be distinguished into two natural classes; and that in sleep, faintness, and insensibility, the eye-ball is given up to the one, and in watchfulness, and the full exercise of the organ, it is given up to the influence of the other class of muscles: and finally, that the consideration of these natural conditions of the eye explains its changes as symptomatic of disease, or as expressive of passion.

Motions of the eye-ball and eye-lids.

Two objects are attained through the motion of the eye-ball. First, the controul and direction of the eye to objects; secondly, the preservation of the organ itself, either by withdrawing the surface from injury, or by the removal of what is offensive to it. Without keeping this distinction before us, we shall not easily discover the uses of the parts.

There is a motion of the eye-ball, which, from its rapidity, has escaped observation. At the instant in which the eye-lids are closed, the eye-ball makes a movement which raises the cornea under the upper eye-lid.

If we fix one eye upon an object, and close the other eye with the finger in such a manner as to feel the convexity of the cornea through the eye-lid, when we shut the eye that is open, we shall feel that the cornea of the other eye is instantly elevated; and that it thus rises and falls in sympathy with the eye that is closed and opened. This change of the position of the eye-ball takes place during the most rapid winking motions of the eye-lids. When a dog was deprived of the power of closing the eye-lids of one eye by the division of the nerve of the eye-lids, the eye did not cease to turn up when he was threatened, and when he winked with the eye-lids of the other side.

Nearly the same thing I observed in a girl whose eye-lids were attached to the surrounding skin, owing to a burn; for the fore part of the eye-ball being completely uncovered, when she would have winked, instead of the eyelids descending, the eye-balls were turned up, and the cornea was moistened by coming into contact with the mouths of the lacrymal ducts.

The purpose of this rapid insensible motion of the eye-ball will be understood on observing the form of the eye-lids and the place of the lacrymal gland. The margins of the eye-lids are flat, and when they meet, they touch only at their outer edges, so that when closed there is a gutter left between them and the cornea. If the eye-ball were to remain without motion, the margins of the eye-lids would meet in such a manner on the surface of the cornea, that a certain portion would be left untouched, and the eye would have no power of clearing off what obscured the vision, at that principal part of the lucid cornea which is in the very axis of the eye; and if the tears flowed they would be left accumulated on the centre of the cornea, and winking, instead of clearing the eye, would suffuse it. To avoid these effects, and to sweep and clear the surface of the cornea, at the same time that the eye-lids are closed, the eye-ball revolves, and the cornea is rapidly elevated under the eye-lid.

Another effect of this motion of the eye-ball is to procure the discharge from the lacrymal ducts; for by the simultaneous ascent of the cornea, and the descent of the upper eye-lid, the membrane on which the ducts open is stretched, and the effect is like the elongation of the nipple, facilitating the discharge of tears.

By the double motion, the descent of the eye-lid and the ascent of the cornea at the same time, the rapidity with which the eye escapes from injury is encreased. Even creatures which have imperfect eye-lids, as fishes, by possessing this rapid revolving motion of the eye, avoid injury and clear off impurities.

I may observe in passing, that there is a provision for the

preservation of the eye, in the manner in which the eye-lids close, which has not been noticed; while the upper eye-lid falls, the lower eye-lid is moved towards the nose. This is a part of that curious provision for collecting offensive particles towards the inner corner of the eye. If the edges of the eye-lids be marked with black spots, it will be seen that when the eye-lids are opened and closed, the spot on the upper eye-lid will descend and rise perpendicularly, while the spot on the lower eye-lid will play horizontally like a shuttle.

To comprehend certain actions of the muscles of the eye, we must remember that the caruncle and membrane called *semilunaris*, seated in the inner corner of the eye, are for ridding the eye of extraneous matter, and are in fact, for the same purpose with that apparatus which is more perfect and appropriate in beasts and birds.

The course of our enquiry makes some observation of these parts necessary.

In quadrupeds there is a gland for secreting a glutinous and adhesive fluid, which is seated on that side of the orbit next the nose; it is quite distinct from the lacrymal gland; it is squeezed by an apparatus of muscles, and the fluid exudes upon the surface of the third eye-lid. This third eye-lid is a very peculiar part of the apparatus of preservation. It is a thin cartilage, the posterior part of which is attached to an elastic body. This body is lodged in a division or depression of the orbit on the side towards the nose. When the eye is excited, the eye-ball is made to press on the elastic body and force it out of its recess or socket; the consequence of which is the protrusion of the cartilaginous

third eye-lid, or *haw*, as it is termed in the horse. By this mechanism the third eye-lid is made to sweep rapidly over the surface of the cornea, and by means of the glutinous fluid with which its surface is bedewed, it attaches and clears away offensive particles.

In birds, the eye is an exquisitely fine organ, and still more curiously, and as we might be tempted to say, artificially protected. The third eye-lid is more perfect; it is membranous and broad, and is drawn over the surface of the eye by means of two muscles which are attached to the back part of the eye-ball, and by a long round tendon, that makes a course of nearly three parts of the circumference of the ball. The lacrymal gland is small, and seated low, but the mucous gland is of great size, and seated in a cavity deep and large, and on the inside of the orbit. As the third eye-lid is moved by an apparatus which cannot squeeze the mucous gland at the same time that the eye-lid is moved, as in quadrupeds, the oblique muscles are particularly provided to draw the eye-ball against the gland, and to force out the mucus on the surface of the third eye-lid. It flows very copiously; and this is probably the reason of the smallness of the proper lacrymal gland which lies on the opposite side of the orbit.

We already see two objects attained through the motion of these parts: the moistening the eye with the clear fluid of the lacrymal gland, and the extraction or protrusion of offensive particles.

There is another division of this subject no less curious; the different conditions of the eye during the waking and sleeping state, remain to be considered. If we approach a person in disturbed sleep when the eye-lids are a little apart,

we shall not see the pupil nor the dark part of the eye, as we should were he awake, for the cornea is turned upwards under the upper eye-lid. If a person be fainting, as insensibility comes over him the eyes cease to have speculation; that is they want direction, and are vacant, and presently the white part of the eye is disclosed by the revolving of the eye-ball upwards. So it is on the approach of death; for, although the eye-lids be open, the pupils are in part hid, being turned up with a seeming agony, which however is the mark of encreasing insensibility.

It will now be admitted that the variety of motions to which the eye is subjected, require the complication of muscles which we find in the orbit, and it must be obvious to the most casual observer, that unless these various offices and different conditions of the eye be considered, it will be in vain to attempt an accurate classification of the muscles of the orbit.

Of the actions of the muscles of the eye, and their natural classification.

The muscles attached to the eye-ball are in two classes, the recti and obliqui. The recti muscles are four in number, and come from the bottom of the orbit, and run a straight course forwards and outwards; they embrace the eye-ball, and are inserted at four cardinal points into it. The obliqui are two muscles having a direction backwards and outwards;*

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* We may say so, for although the superior oblique muscle comes from the back of the orbit, yet, by passing through the trochlea, it has a course backwards and outwards to its insertion.

they embrace the eye-ball, one passing over it obliquely, the other under it obliquely.

That the recti muscles perform the office of directing the axis of the eye, turning it round to every point in the sphere of vision, there are many proofs. In the first place, their origin, course, and insertion, accurately fit them for this office; and they are obviously equal to it, unassisted by other muscles. In the next place, from man down to the cuttle-fish, the voluntary motions of the eyes are the same, and the origin, course, and insertion of these muscles are similar, while the other muscles vary with the change of apparatus which is around the eye.

The oblique muscles of the eye stand contrasted with the recti in every respect, in number, size, and direction. Yet it is a received opinion, that they antagonize the recti, and keep the eye suspended. To this opinion there are many objections. 1. In creatures where the eye is socketed on a cup of cartilage and cannot retract, the oblique muscles are nevertheless present. 2. Where a powerful retractor muscle is bestowed in addition to the recti muscles, the oblique muscles have no additional magnitude given to them. 3. In matter of fact, the human eye cannot be retracted by the united action of the recti as we see quadrupeds draw in their eyes, which is an argument against these muscles being retractors, and therefore against the obliqui being their opponents, to draw it forward.

To these, other objections, no less strong, may be added. We have just found that certain very rapid motions are to be performed by the eyeball; now it can be demonstrated, that a body will be moved in less time by a muscle which is

oblique to the line of motion, than if it lay in the line on which the body moves. If the oblique muscles were either opponents or coadjutors of the recti, there appears no reason why they should be oblique, but the contrary; for as the points of their insertion must move more rapidly than those of the recti, they are unsuitable. On the other hand, that there may be no difference in the time of the action and relaxation of the several classes, we see a reason why one rectus should be opposed by another, and why there being occasion for one oblique, its antagonist should also be oblique.

In proportion as a muscle gains velocity by its obliquity, it loses power; from the obliquity, therefore, of these muscles believed to be opposed to the recti, and from their being two of them to four of the latter, they are disproportioned in strength, and the disproportion proves that the two classes of muscles are not antagonists.

By dissection and experiment it can be proved, that the oblique muscles are antagonists to each other, and that they roll the eye in opposite directions, the superior oblique directing the pupil downwards and outwards, and the inferior oblique directing it upwards and inwards. But it is proved that any two of the recti muscles are equal to the direction of the pupil in the diagonal between them, and there is no reason why an additional muscle should be given, to direct the pupil upwards and inwards more than upwards and outwards, or downwards and inwards. It is evident then, that the oblique muscles are not for assisting the recti in directing the eye to objects, but that they must have some other appropriate office. If we proceed farther, it must be by experiment.

Experimental enquiry into the action of these muscles.

I. I divided the *superior rectus* or *attollens* in a rabbit, and felt something like disappointment on observing the eye remain stationary. Shortly afterwards, on looking to the animal while it was feeding, I saw the pupil depressed, and that the animal had no power of raising it.

The explanation I conceive to be this: during the experiment the eye was spasmodically fixed by the general action of the muscles, and particularly by the powerful retractor, a muscle peculiar to quadrupeds. But on the spasm relaxing, and when the eye was restored to the influence of the voluntary muscles, the recti, the voluntary power of raising the eye being lost by the division of the superior muscle, the eye was permanently depressed.

II. Wishing to ascertain if the oblique muscles contract to force the eye-ball laterally towards the nose, I put a fine thread round the tendon of the superior oblique muscle of a rabbit, and appended a glass bead to it of a weight to draw out the tendon a little. On touching the eye with a feather, I had the pleasure of seeing the bead drawn up. And on repeating the experiment, the thread was forcibly drawn through my fingers.

By experiments made carefully in the dead body, (having distended the eye-ball by dropping mercury into it to give it its full globular figure) I had found that the action of the superior oblique muscle is to turn the pupil downwards and outwards, and that the inferior oblique just reverses this motion of the eye. In the above experiment there is abundance of proof that the superior oblique muscle acted, and yet the

pupil was not turned downwards and outwards, therefore both oblique muscles must have been in action. Their combined action draws the eye-ball towards the nose.

In the violent spasmodic affection of the eye, when it is painfully irritated, I believe that all the muscles, both of the eye-ball and eye-lids, are excited. In quadrupeds, I have ascertained that the oblique muscles act when the haw is protruded, but I have also found that the retractor oculi alone, is capable of forcing forwards the haw.

But quadrupeds having an additional apparatus of muscles to those of the human eye, are not suited for experiments intended to illustrate the motions of our eyes. The monkey has the same muscles of the eye with man.

III. I cut across the tendon of the superior oblique muscle of the right eye of a monkey. He was very little disturbed by this experiment, and turned round his eyes with his characteristic enquiring looks, as if nothing had happened to affect the eye.

IV. I divided the lower oblique muscle of the eye of a monkey. The eye was not, in any sensible manner, affected; the voluntary motions were perfect after the operation.

V. On holding open the eyes of the monkey, which had the superior oblique muscle of the right eye divided, and waving the hand before him, the right eye turned upwards and inwards, while the other eye had a scarcely perceptible motion in the same direction. When the right eye was thus turned up, he seemed to have a difficulty in bringing it down again.

From these experiments it is proved, that the division of the oblique muscles does not in any degree affect the voluntary motions by which the eye is directed to objects.

This cannot however be said of the involuntary winking motions of the eyes. We have seen that in winking to avoid injury, the oblique muscles were in operation; and that the inferior oblique muscle gained in the power of elevating the eye-ball by the division of the superior oblique, its opponent.*

On the two conditions of the eye, its state of rest, and of activity.

The eye is subject to two conditions: a state of rest with entire oblivion of sensation, and a state of watchfulness, during which both the optic nerve and the nerve of voluntary motion are in activity. When the eye is at rest, as in sleep, or even when the eye-lids are shut, the sensation on the retina being then neglected, the voluntary muscles resign their office, and the involuntary muscles draw the pupil under the upper eye-lid. This is the condition of the organ during perfect repose.

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* Since this paper was read, a case has occurred in the Middlesex Hospital, under the care of my colleague, Dr. Macmichael, which shows the consequences of the eye and eye-lids being rendered immoveable. In this case the surface of the eye is totally insensible, and the eye remains fixed, and directed straight forwards, whilst the vision is entire. The outward apparatus being without sensibility and motion, and the surface not cleared of irritating particles, inflammation has taken place, and the cornea is becoming opaque; thus proving the necessity of the motions of the eye to the preservation of the organ. Another curious circumstance, illustrative of the observations made above, is, that when both eyes are shut, the eye affected continues to be sensible of a red light coming through the eye-lid, whilst the sound eye is in darkness. The reason of this I apprehend to be: the eye which possesses its natural motions is turned up, but the eye which continues fixed, looking forwards, receives the light through the transparent eye-lid; and thus it appears that the dropping of the eye-lid would make an imperfect curtain, if unaccompanied by the turning up of the eye-ball during repose.

The interest of this case will be encreased by the considerations in the Second Part of this Paper.

On the other hand, there is an inseparable connection between the exercise of the sense of vision and the exercise of the voluntary muscles of the eye. When an object is seen, we enjoy two senses; there is an impression upon the retina; but we receive also the idea of position or relation which it is not the office of the retina to give. It is by the consciousness of the degree of effort put upon the voluntary muscles, that we know the relative position of an object to ourselves. The relation existing between the office of the retina and of the voluntary muscles, may be illustrated in this manner.

Let the eyes be fixed upon an illuminated object until the retina be fatigued, and in some measure exhausted by the image, then closing the eyes, the figure of the object will continue present to them: and it is quite clear that nothing can change the place of this impression on the retina. But notwithstanding that the impression on the retina cannot be changed, the idea thence arising may. For by an exertion of the voluntary muscles of the eye-ball, the body seen will appear to change its place, and it will, to our feeling, assume different positions according to the muscle which is exercised. If we raise the pupil, we shall see the body elevated, or if we depress the pupil, we shall see the body placed below us; and all this takes place while the eye-lids are shut, and when no new impression is conveyed to the retina. The state of the retina is here associated with a consciousness of muscular exertion; and it shows that vision in its extended sense is a compound operation, the idea of position of an object having relation to the activity of the muscles.

We may also show, by varying this experiment, that an agitated state of the muscles, or a state of action where the muscles are at variance or confused, affects the idea of the

mage. If we look on the luminous body so as to make this impression on the retina, and then cover the face so as to exclude the light, keeping the eye-lids open, and if we now squint, or distort the eyes, the image which was vividly impressed upon the retina instantly disappears as if it were wiped out. Does not this circumstance take place, because the condition of the muscles thus unnaturally produced, being incongruous with the exercise of the retina, disturbs its operation?

If we move the eye by the voluntary muscles, while this impression continues on the retina, we shall have the notion of place or relation raised in the mind; but if the motion of the eye-ball be produced by any other cause, by the involuntary muscles, or by pressure from without, we shall have no corresponding change of sensation.

If we make the impression on the retina in the manner described, and shut the eyes, the image will not be elevated, although the pupils be actually raised, as it is their condition to be when the eyes are shut, because there is here no sense of voluntary exertion. If we sit at some distance from a lamp which has a cover of ground glass, and fix the eye on the centre of it, and then shut the eye and contemplate the phantom in the eye; and if, while the image continues to be present of a fine blue colour, we press the eye aside with the finger, we shall not move that phantom or image, although the circle of light produced by the pressure of the finger against the eye-ball moves with the motion of the finger.

May not this be accounted for in this manner: the motion produced in the eye-ball not being performed by the appropriate organs, the voluntary muscles, it conveys no sensation of

change to the sensorium, and is not associated with the impression on the retina, so as to affect the idea excited in the mind? It is owing to the same cause that, when looking on the lamp, by pressing one eye, we can make two images, and we can make the one move over the other. But, if we have received the impression on the retina so as to leave the phantom visible when the eye-lids are shut, we cannot, by pressing one eye, produce any such effect. We cannot, by any degree of pressure, make that image appear to move, but the instant that the eye moves by its voluntary muscles, the image changes its place; that is, we produce the two sensations necessary to raise this idea in the mind; we have the sensation on the retina combined with the consciousness or sensation of muscular activity.

These experiments and this explanation of the effect of the associated action of the voluntary muscles of the eye-ball, appear to me to remove an obscurity in which this subject has been left by the latest writers. In a most scientific account of the eye and of optics, lately published, it is said on this question, " we know nothing more than that the mind residing, as it were, in every point of the retina, refers the impression made upon it, at each point, to a direction coinciding with the last portion of the ray which conveys the impression." The same author says " KEPLER justly ascribed erect vision from an inverted image to an operation of the mind by which it traces the rays back to the pupil, and thus refers the lower part of the image to the upper side of the eye." What can be here meant by the mind following back the ray through the humors of the eye? It might as well follow the ray out of the eye, and like the spider feel along

the line. A much greater authority says we puzzle ourselves without necessity. "We call that the lower end of an object which is next the ground." No one can doubt that the obscurity here, is because the author has not given himself room to illustrate the subject by his known ingenuity and profoundness. But it appears to me, that the utmost ingenuity will be at a loss to devise an explanation of that power by which the eye becomes acquainted with the position and relation of objects, if the sense of muscular activity be excluded, which accompanies the motion of the eye-ball.

Let us consider how minute and delicate the sense of muscular motion is by which we balance the body, and by which we judge of the position of the limbs, whether during activity or rest. Let us consider how imperfect the sense of touch would be, and how little of what is actually known through the double office of muscles and nerves, would be attained by the nerve of touch alone, and we shall be prepared to give more importance to the recti muscles of the eye, in aid of the sense of vision: to the offices performed by the frame around the eye-ball in aid of the instrument itself.

Of the expression of the eye, and of the actions of the oblique muscles in disease.

If, as I have alleged, the uses of the oblique muscles of the eye have been misunderstood, and if, as I hope presently to prove, the distinctions of the nerves have been neglected, the symptoms of disease, and the sources of expression in the eye, must remain to be explained.

During sleep, in oppression of the brain, in faintness, in debility after fever, in hydrocephalus, and on the approach of death, the pupils of the eyes are elevated. If we open the

eye-lids of a person during sleep or insensibility, the pupils will be found elevated. Whatever be the cause of this, it will be found that it is also the cause of the expression in sickness, and pain, and exhaustion, whether of body or mind: for then the eye-lids are relaxed and fallen, and the pupils elevated so as to be half covered by the upper eye-lid. This condition of the eye during its insensible unexercised state, we are required to explain.

It is a fact familiar to pathologists, that when debility arises from affection of the brain, the influence is greatest on those muscles which are, in their natural condition, most under the command of the will. We may perceive this in the progressive stages of debility in the drunkard, when successively the muscles of the tongue, the eyes, the face, the limbs, become unmanageable; and, under the same circumstances, the muscles which have a double office, as those of the chest, lose their voluntary motions, and retain their involuntary motions, the force of the arms is gone long before the action of breathing is affected.

If we transfer this principle, and apply it to the muscles of the eye, we shall have an easy solution of the phenomena above enumerated. The recti are voluntary muscles, and they suffer debility before the oblique muscles are touched by the same condition; and the oblique muscles prevailing, roll the eye.

If it be farther asked, why does the eye roll upwards and inwards? We have to recollect, that this is the natural condition of the eye, its position when the eye-lids are shut and the light excluded, and the recti at rest and the obliqui balanced.

Although I am aware that medical histories do not often lead

to the improvement of strict science, yet I am tempted to describe the condition of a patient now under my care, because it exhibits a succession of those phenomena which we seek to explain. He presented himself to me in the hospital, with a distinct squint, the left eye being distorted from the object. On the eye-lid of the right eye there was a deep and open ulcer; the man was in danger of losing the right eye, and required prompt assistance; but before he could be brought under the influence of medicine, the inflamed sore became deeper and the cornea opaque. The superior rectus muscle being, as I suppose, injured by the encreasing depth of the sore, the pupil became permanently depressed. The sight of the right eye being now lost, the left eye came into use; it was directed with precision to objects, he had no difficulty in using it, and it daily became stronger.

After a few weeks, medicine having had its influence, the sore on the upper eye-lid of the right eye healed, the inflammation and opacity of the eye gradually diminished, the light became again visible to him; first it was yellow, and then a deep purple. And now the muscles resumed their influence, and the eye was restored to parallel motion with the other, and so as considerably to embarrass the vision. But the inflammation of the upper eye-lid had been so great, as considerably to diminish its mobility; and what appeared most extraordinary, the lower eye-lid assumed the office of the upper one, and a very unusual degree of motion was remarked in it. It was depressed when he attempted to open the eye, and elevated and drawn towards the nose, when he closed the eye. But the upper eye-lid was not only stiff, but diminished in breadth; so that notwithstanding the remarkable elevation of the lower

eye-lid, their margins were not brought together, and we could perceive the motion of the eye-ball; in his attempt to close the eye we saw the pupil elevated, and the white part of the eye exposed.

I shall now attempt the explanation of some of these phenomena:

The impression upon the left eye had been weak from infancy, and the retina being unexercised, the recti or voluntary muscles wanted their excitement, and were deficient in activity; the involuntary muscles therefore prevailed, and the pupil was turned upwards and inwards, and consequently removed from the axis of the other eye. But when that other eye became obscured, the left eye being the only inlet to sensation, the attention became directed to the impression on the retina, the voluntary muscles were excited to activity, and they brought the eye to bear upon objects. This eye improved daily, because the natural exercise of a part is its stimulus to perfection, both in function and in growth. When the right eye became transparent and the light was admitted, the voluntary muscles of that eye partook of their natural stimulus, and commenced that effort in search of the object, which in the course of a few days brought the eye to its proper axis, and both eyes to parallelism.

The next thing that attracts our attention in this short narrative, is the revolving of the eye-ball. It has been explained in a former part of the paper, that when the eye-lids are shut, the recti or voluntary muscles resign their office, and the inferior oblique muscle gains power, and the eye-ball traverses so as to raise the pupil. It will not have escaped observation, that the pupil of this eye was depressed, and

could not be elevated for the purpose of vision, owing, as we have supposed, to the injury of the rectus attollens, at the same time that it was thus raised involuntarily, in the attempt to shut the eye; a proof that this insensible motion is performed by the lower oblique muscle, and not the superior rectus muscle.

The circumstance of the lower eye-lid assuming the functions of the upper one, and moving like the lower eye-lid of a bird, reminds me of an omission in the account of authors. They have sought for a depressor of the inferior eye-lid, which has no existence, and is quite unnecessary; for the motion of the *M. elevator palpebræ superioris* opens wide the eye-lids, and depresses the lower eye-lid, at the same time that it elevates the upper eye-lid. If we put the finger on the lower eye-lid when the eye is shut, and then open the eye, we shall feel that during this action the eye-ball is pushed outwards; and we may observe, that the lower eye-lid is so adapted as to slip off the convex surface of the ball, and is consequently depressed. The reason of this is, that the muscle which raises the upper eye-lid passes over a considerable part of the upper and back part of the eye-ball, and the origin and insertion of the muscle being under the highest convexity of the ball, that body must be pressed forwards in proportion to the resistance of the upper eye-lid to rise. In the preceding case the upper eye-lid being stiff and unyielding, both the origin and the insertion of the *elevator palpebræ* became fixed points; consequently, the action of the muscle fell entirely on the eye-ball itself, whereby it was forced downwards and forwards in an unusual manner, and so depressed the lower eye-lid to an unusual degree. Thus the muscle became a *depressor* of the inferior eye-lid, instead of an *elevator* of the

upper eye-lid! The motion of elevation in the lower eye-lid was of course performed by an encreased action of the lower portion of the *orbicularis palpebrarum.*

The Author has to regret that these minute circumstances regarding the action of the muscles of the eye have led him to so great a length; he hopes they are not altogether without interest in themselves, while the discussion will afford him secure ground for establishing an arrangement of the nerves of the eye, and will enable him to distinguish them according to their uses.

EXPLANATION OF PLATE XXI.

Fig. 1. The muscles of the eye seen in front.

A. B. C. D. The recti muscles; voluntary muscles.

E. The superior oblique muscle or trochlearis.

a. The trochlea cut off from the bone and left attached to the tendon. It is a loop through which the tendon runs.

b. The tendon of the trochlearis muscle expanding and running to its insertion.

G. The inferior oblique muscle. It is seen, like the tendon of the superior oblique, to run backwards and outwards.

Fig. 2. The muscles of the eye seen in profile.

A. B. . D. Three of the recti muscles. They arise together from the periosteum of the bottom of the orbit, and are inserted into the anterior part of the sclerotic coat of the eye.

E. The superior oblique muscle, or trochlearis.

a. The trochlea.

b. The reflected tendon inserted into the back and outer part of the sclerotic coat.

G. The inferior oblique muscle.

c. Its origin from the anterior part of the orbit.

d. Its insertion into the back and outer part of the eye-ball.

Fig. 1.

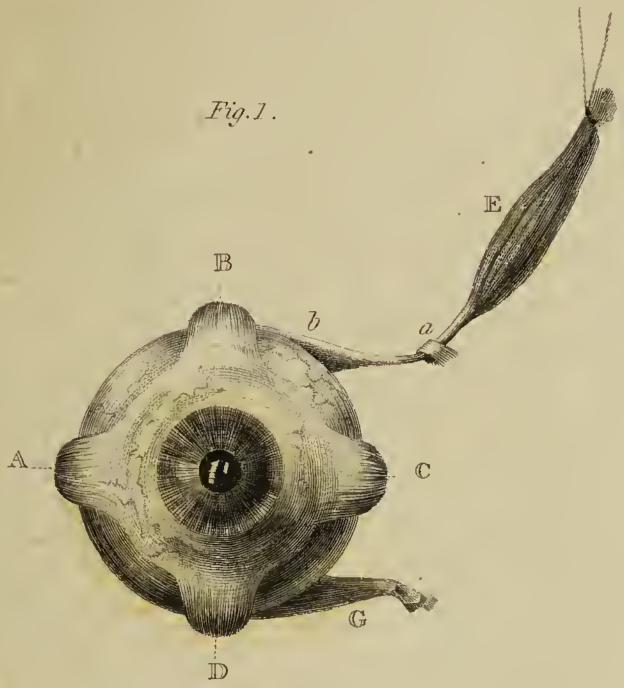
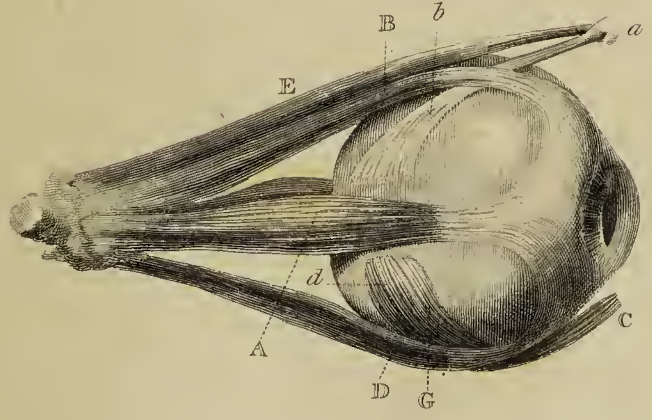


Fig. 2.



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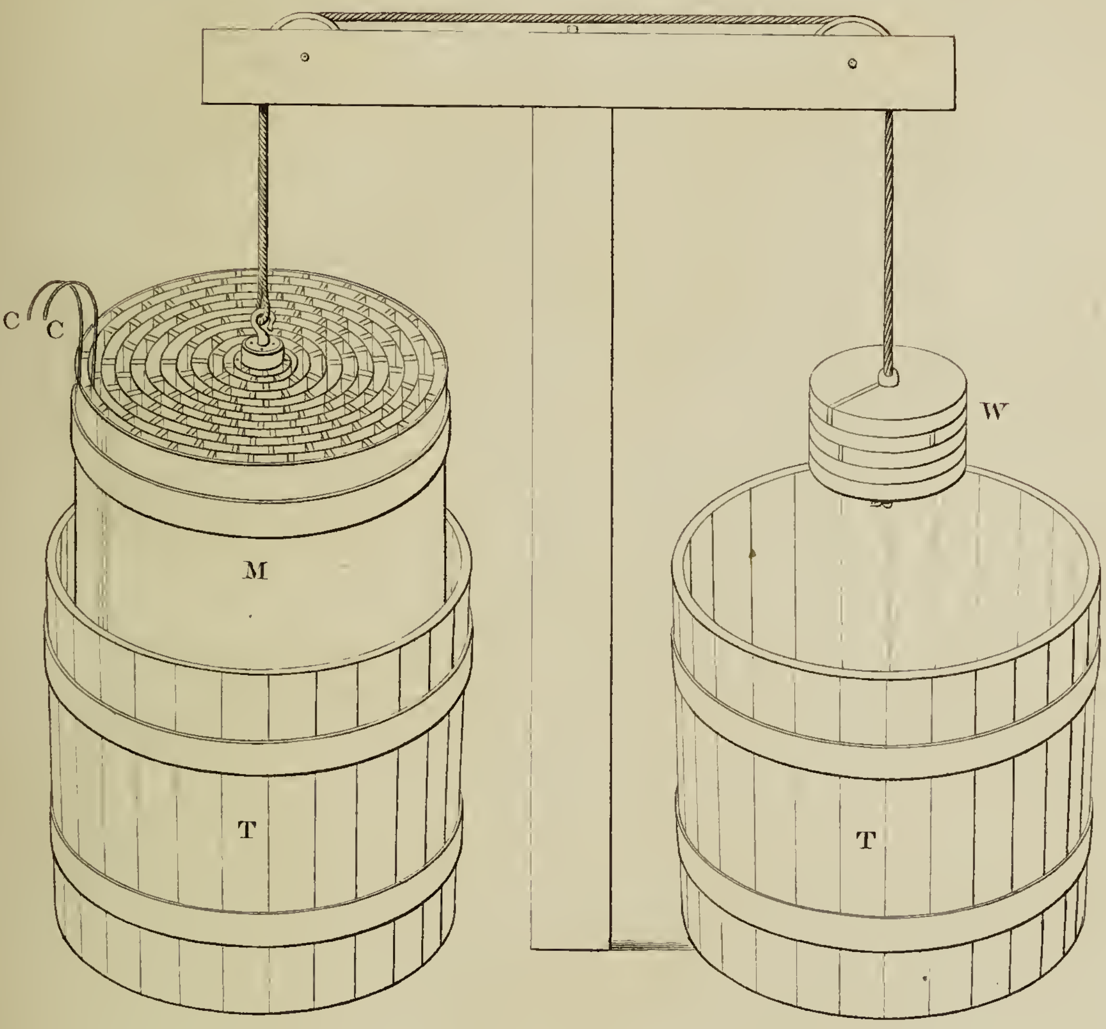

M. *Electromagnetic Apparatus.*

C C. *The Conductors.*

W. *The Counterpoise Weight.*

T T. *The Tubs, one for Acid, and one for Water.*

Jˢ Basire sculpᵗ.

XVI. *An account of an apparatus on a peculiar construction for performing electro-magnetic experiments.* *By* W. H. PEPYS, *Esq. F. R. S.*

Read April 10, 1823.

THIS apparatus [Pl. XXII.] was made under my directions for the London Institution. It consists of two plates, each fifty feet in length, and two feet in width; the one copper, and the other zinc, making a superficial surface of four hundred feet. They are rolled or wrapped round a cylinder of wood with three strands or ropes of horse hair between each plate, to prevent contact of the metals; and to maintain these in their situation, notched sticks are occasionally introduced in the rolling. Two conductors of copper near three fourths of an inch in thickness are secured to the end of each plate, from which the power is dispensed upon immersion in the acid.

To allow of the free use of so bulky an instrument, it is suspended by ropes and pullies, with a counterpoise weight, to allow its immersion in a tub of dilute acid, or when not in use, in one of water; it requires about fifty-five gallons of fluid, and the strength of the solution used has been about one-fortieth of strong nitrous acid.

Upon immersing the instrument in the dilute acid, and uniting the two conductors, magnetic needles on *their stands* were very sensibly affected for five feet from the conductors.

Cylindrical bars of steel placed in the interior of a glass tube, surrounded by a spiral of wire, and forming part of

the circuit, were made powerfully magnetic, (so as to be suspended from each other). When the tube and spiral were placed perpendicularly, steel cylinders or bars inserted were supported entirely by the attraction; one of these cylinders weighing 272 grains; when the contact was broken, the cylinder fell from its gravity, but instantly rushed into its former place upon the contact being made. The copper *plate* conductor gave the north magnetic pole, and the zinc plate conductor gave the south magnetic pole.

This apparatus, as might be expected, has no intensity as a chemical agent, not even giving a spark with charcoal. But an extraordinary proof of its low intensity, is, that leaves or laminæ of the metals are not deflagrated, and very small portions of wire are ignited.

XVII. *On the condensation of several gases into liquids. By Mr.* FARADAY, *Chemical Assistant in the Royal Institution. Communicated by Sir* HUMPHRY DAVY, *Bart. Pres. R. S.*

Read April 10, 1823.

I HAD the honour, a few weeks since, of submitting to the Royal Society a paper on the reduction of chlorine to the liquid state. An important note was added to the paper by the President, on the general application of the means used in this case to the reduction of other gaseous bodies to the liquid state; and in illustration of the process, the production of liquid muriatic acid was described. Sir HUMPHRY DAVY did me the honour to request I would continue the experiments, which I have done under his general direction, and the following are some of the results already obtained:

Sulphurous Acid.

Mercury and concentrated sulphuric acid were sealed up in a bent tube, and, being brought to one end, heat was carefully applied, whilst the other end was preserved cool by wet bibulous paper. Sulphurous acid gas was produced where the heat acted, and was condensed by the sulphuric acid above; but, when the latter had become saturated, the sulphurous acid passed to the cold end of the tube, and was condensed into a liquid. When the whole tube was cold, if the sulphurous acid were returned on to the mixture of

sulphuric acid and sulphate of mercury, a portion was reabsorbed, but the rest remained on it without mixing.

Liquid sulphurous acid is very limpid and colourless, and highly fluid. Its refractive power, obtained by comparing it in water and other media, with water contained in a similar tube, appeared to be nearly equal to that of water. It does not solidify or become adhesive at a temperature of 0° F. When a tube containing it was opened, the contents did not rush out as with explosion, but a portion of the liquid evaporated rapidly, cooling another portion so much as to leave it in the fluid state at common barometric pressure. It was however rapidly dissipated, not producing visible fumes, but producing the odour of pure sulphurous acid, and leaving the tube quite dry. A portion of the vapour of the fluid received over a mercurial bath, and examined, proved to be sulphurous acid gas. A piece of ice dropped into the fluid instantly made it boil, from the heat communicated by it.

To prove in an unexceptionable manner that the fluid was pure sulphurous acid, some sulphurous acid gas was carefully prepared over mercury, and a long tube perfectly dry, and closed at one end, being exhausted, was filled with it; more sulphurous acid was then thrown in by a condensing syringe, till there were three or four atmospheres; the tube remained perfectly clear and dry, but on cooling one end to 0°, the fluid sulphurous acid condensed, and in all its characters was like that prepared by the former process.

A small gage was attached to a tube in which sulphurous acid was afterwards formed, and at a temperature of 45° F. the pressure within the tube was equal to three atmospheres, there being a portion of liquid sulphurous acid present: but

as the common air had not been excluded when the tube was sealed, nearly one atmosphere must be due to its presence, so that sulphurous acid vapour exerts a pressure of about two atmospheres at 45° F. Its specific gravity was nearly 1.42.*

Sulphuretted hydrogen.

A tube being bent, and sealed at the shorter end, strong muriatic acid was poured in through a small funnel, so as nearly to fill the short leg without soiling the long one. A piece of platinum foil was then crumpled up and pushed in, and upon that were put fragments of sulphuret of iron, until the tube was nearly full. In this way action was prevented until the tube was sealed. If it once commences, it is almost impossible to close the tube in a manner sufficiently strong, because of the pressing out of the gas. When closed, the muriatic acid was made to run on to the sulphuret of iron, and then left for a day or two. At the end of that time, much proto-muriate of iron had formed, and on placing the

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* I am indebted to Mr. DAVIES GILBERT, who examined with much attention the results of these experiments, for the suggestion of the means adopted to obtain the specific gravity of some of these fluids. A number of small glass bulbs were blown and hermetically sealed; they were then thrown into alcohol, water, sulphuric acid, or mixtures of these, and when any one was found of the same specific gravity as the fluid in which it was immersed, the specific gravity of the fluid was taken: thus a number of hydrometrical bulbs were obtained; these were introduced into the tubes in which the substances were to be liberated; and ultimately, the dry liquids obtained, in contact with them. It was then observed whether they floated or not, and a second set of experiments were made with bulbs lighter or heavier as required, until a near approximation was obtained. Many of the tubes burst in the experiments, and in others difficulties occurred from the accidental fouling of the bulb by the contents of the tube. One source of error may be mentioned in addition to those which are obvious, namely, the alteration of the bulk of the bulb by its submission to the pressure required to keep the substance in the fluid state.

clean end of the tube in a mixture of ice and salt, warming the other end if necessary by a little water, sulphuretted hydrogen in the liquid state distilled over.

The liquid sulphuretted hydrogen was colourless, limpid, and excessively fluid. Ether, when compared with it in similar tubes, appeared tenacious and oily. It did not mix with the rest of the fluid in the tube, which was no doubt saturated, but remained standing on it. When a tube containing it was opened, the liquid immediately rushed into vapour; and this being done under water, and the vapour collected and examined, it proved to be sulphuretted hydrogen gas. As the temperature of a tube containing some of it rose from 0° to 45°, part of the fluid rose in vapour, and its bulk diminished; but there was no other change: it did not seem more adhesive at 0° than at 45°. Its refractive power appeared to be rather greater than that of water; it decidedly surpassed that of sulphurous acid. A small gage being introduced into a tube in which liquid sulphuretted hydrogen was afterwards produced, it was found that the pressure of its vapour was nearly equal to 17 atmospheres at the temperature of 50°.

The gages used were made by drawing out some tubes at the blow-pipe table until they were capillary, and of a trumpet form; they were graduated by bringing a small portion of mercury successively into their different parts; they were then sealed at the fine end, and a portion of mercury placed in the broad end; and in this state they were placed in the tubes, so that none of the substances used, or produced, could get to the mercury, or pass by it to the inside of the gage. In estimating the number of atmospheres, one has always been subtracted for the air left in the tube.

The specific gravity of sulphuretted hydrogen appeared to be 0.9.

Carbonic acid.

The materials used in the production of carbonic acid, were carbonate of ammonia and concentrated sulphuric acid; the manipulation was like that described for sulphuretted hydrogen. Much stronger tubes are however required for carbonic acid than for any of the former substances, and there is none which has produced so many or more powerful explosions. Tubes which have held fluid carbonic acid well for two or three weeks together, have, upon some increase in the warmth of the weather, spontaneously exploded with great violence; and the precautions of glass masks, goggles, &c. which are at all times necessary in pursuing these experiments, are particularly so with carbonic acid.

Carbonic acid is a limpid colourless body, extremely fluid, and floating upon the other contents of the tube. It distils readily and rapidly at the difference of temperature between 32° and 0°. Its refractive power is much less than that of water. No diminution of temperature to which I have been able to submit it, has altered its appearance. In endeavouring to open the tubes at one end, they have uniformly burst into fragments, with powerful explosions. By inclosing a gage in a tube in which fluid carbonic acid was afterwards produced, it was found that its vapour exerted a pressure of 36 atmospheres at a temperature of 32°.

It may be questioned, perhaps, whether this and other similar fluids obtained from materials containing water, do not contain a portion of that fluid; in as much as its absence has not been proved, as it may be with chlorine, sulphurous

acid, cyanogen, and ammonia. But besides the analogy which exists between the latter and the former, it may also be observed in favour of their dryness, that any diminution of temperature causes the deposition of a fluid from the atmosphere, precisely like that previously obtained; and there is no reason for supposing that these various atmospheres, remaining as they do in contact with concentrated sulphuric acid, are not as dry as atmospheres of the same kind would be over sulphuric acid at common pressure.

Euchlorine.

Fluid euchlorine was obtained by inclosing chlorate of potash and sulphuric acid in a tube, and leaving them to act on each other for 24 hours. In that time there had been much action, the mixture was of a dark reddish brown, and the atmosphere of a bright yellow colour. The mixture was then heated up to 100°, and the unoccupied end of the tube cooled to 0°; by degrees the mixture lost its dark colour, and a very fluid ethereal looking substance condensed. It was not miscible with a small portion of the sulphuric acid which lay beneath it; but when returned on to the mass of salt and acid, it was gradually absorbed, rendering the mixture of a much deeper colour even than itself.

Euchlorine thus obtained is a very fluid transparent substance, of a deep yellow colour. A tube containing a portion of it in the clean end, was opened at the opposite extremity; there was a rush of euchlorine vapour, but the salt plugged up the aperture: whilst clearing this away, the whole tube burst with a violent explosion, except the small end in a cloth in my hand, where the euchlorine previously lay, but the fluid had all disappeared.

Nitrous oxide.

Some nitrate of ammonia, previously made as dry as could be by partial decomposition, by heat in the air, was sealed up in a bent tube, and then heated in one end, the other being preserved cool. By repeating the distillation once or twice in this way, it was found, on after-examination, that very little of the salt remained undecomposed. The process requires care. I have had many explosions occur with very strong tubes, and at considerable risk.

When the tube is cooled, it is found to contain two fluids, and a very compressed atmosphere. The heavier fluid on examination proved to be water, with a little acid and nitrous oxide in solution; the other was nitrous oxide. It appears in a very liquid, limpid, colourless state; and so volatile that the warmth of the hand generally makes it disappear in vapour. The application of ice and salt condenses abundance of it into the liquid state again. It boils readily by the difference of temperature between 50° and 0°. It does not appear to have any tendency to solidify at —10°. Its refractive power is very much less than that of water, and less than any fluid that has yet been obtained in these experiments, or than any known fluid. A tube being opened in the air, the nitrous oxide immediately burst into vapour. Another tube opened under water, and the vapour collected and examined, it proved to be nitrous oxide gas. A gage being introduced into a tube, in which liquid nitrous oxide was afterwards produced, gave the pressure of its vapour as equal to above 50 atmospheres at 45°.

Cyanogen.

Some pure cyanuret of mercury was heated until perfectly dry. A portion was then inclosed in a green glass tube, in the same manner as in former instances, and being collected to one end, was decomposed by heat, whilst the other end was cooled. The cyanogen soon appeared as a liquid: it was limpid, colourless, and very fluid; not altering its state at the temperature of 0°. Its refractive power is rather less, perhaps, than that of water. A tube containing it being opened in the air, the expansion within did not appear to be very great; and the liquid passed with comparative slowness into the state of vapour, producing great cold. The vapour, being collected over mercury, proved to be pure cyanogen.

A tube was sealed up with cyanuret of mercury at one end, and a drop of water at the other; the fluid cyanogen was then produced in contact with the water. It did not mix, at least in any considerable quantity, with that fluid, but floated on it, being lighter, though apparently not so much so as ether would be. In the course of some days, action had taken place, the water had become black, and changes, probably such as are known to take place in an aqueous solution of cyanogen, occurred. The pressure of the vapour of cyanogen appeared by the guage to be 3.6 or 3.7 atmospheres at 45° F. Its specific gravity was nearly 0.9.

Ammonia.

In searching after liquid ammonia, it became necessary, though difficult, to find some dry source of that substance; and I at last resorted to a compound of it, which I had occa-

sion to notice some years since with chloride of silver.[*] When dry chloride of silver is put into ammoniacal gas, as dry as it can be made, it absorbs a large quantity of it; 100 grains condensing above 130 cubical inches of the gas: but the compound thus formed is decomposed by a temperature of 100° F. or upwards. A portion of this compound was sealed up in a bent tube and heated in one leg, whilst the other was cooled by ice or water. The compound thus heated under pressure fused at a comparatively low temperature, and boiled up, giving off ammoniacal gas, which condensed at the opposite end into a liquid.

Liquid ammonia thus obtained was colourless, transparent, and very fluid. Its refractive power surpassed that of any other of the fluids described, and that also of water itself. From the way in which it was obtained, it was evidently as free from water as ammonia in any state could be. When the chloride of silver is allowed to cool, the ammonia immediately returns to it, combining with it, and producing the original compound. During this action a curious combination of effects takes place: as the chloride absorbs the ammonia, heat is produced, the temperature rising up nearly to 100°; whilst a few inches off, at the opposite end of the tube, considerable cold is produced by the evaporation of the fluid. When the whole is retained at the temperature of 60°, the ammonia boils till it is dissipated and re-combined. The pressure of the vapour of ammonia is equal to about 6.5 atmospheres at 50°. Its specific gravity was 0.76.

[*] Quarterly Journal of Science, vol. V. p. 74.

Muriatic acid,

When made from pure muriate of ammonia and sulphuric acid, liquid muriatic acid is obtained colourless, as Sir Humphry Davy had anticipated. Its refractive power is greater than that of nitrous oxide, but less than that of water; it is nearly equal to that of carbonic acid. The pressure of its vapour at the temperature of 50°, is equal to about 40 atmospheres.

Chlorine.

The refractive power of fluid chlorine is rather less than that of water. The pressure of its vapour at 60° is nearly equal to 4 atmospheres.

Attempts have been made to obtain hydrogen, oxygen, fluoboracic, fluosilicic, and phosphuretted hydrogen gases in the liquid state; but though all of them have been subjected to great pressure, they have as yet resisted condensation. The difficulty with regard to fluoboric gas consists, probably, in its affinity for sulphuric acid, which, as Dr. Davy has shown, is so great as to raise the sulphuric acid with it in vapour. The experiments will however be continued on these and other gases, in the hopes that some of them, at least, will ultimately condense.

XVIII. *On the application of liquids formed by the condensation of gases as mechanical agents.* *By Sir* Humphry Davy, *Bart. Pres. R. S.*

Read April 17, 1823.

One of the principal objects that I had in view, in causing experiments to be made on the condensation of different gaseous bodies, by generating them under pressure, was the hope of obtaining vapours, which, from the facility with which their elastic forces might be diminished or increased, by small decrements or increments of temperature, would be applicable to the same purposes as steam.

As soon as I had obtained muriatic acid in the liquid state, a body which M. Berthollet supposed owed its power of being separated from bases by other acids, only to the facility with which it assumes the gaseous form, I had no doubt, as I mentioned in my last communication, that all the other gases which have weaker affinities or greater densities, and which are absorbable to any extent by water, might be rendered fluid by similar means; and, that the conjecture was founded, has been proved by the experiments made with so much industry and ingenuity by Mr. Faraday, and which I have had the pleasure of communicating to the Society.

The elasticity of vapours in contact with the liquids from which they are produced, under high pressures by high temperatures, such as those of alcohol and water, is known to increase in a much higher ratio than the arithmetical one of the

temperature; but the exact law is not yet determined; and the result is a complicated one, and depends upon circumstances which require to be ascertained by experiment. Thus the ratio of the elastic force, dependent upon pressure, is to be combined with that of the expansive force dependent upon temperature; and the greater loss of radiant heat at high temperatures, and the developement of latent heat in compression, and the necessity for its re-absorption in expansion (as the rationale of the subject is at present understood) must awaken some doubts as to the economical results to be obtained by employing the steam of water under very great pressures, and at very elevated temperatures.

No such doubts, however, can arise with respect to the use of such liquids, as require for their existence even a compression equal to that of the weight of 30 or 40 atmospheres: and where common temperatures, or slight elevations of them, are sufficient to produce an immense elastic force; and when the principal question to be discussed, is whether the effect of mechanical motion is to be most easily produced by an increase or diminution of heat by artificial means.

With the assistance of Mr. FARADAY I have made some experiments on this subject, and the results have answered my most sanguine expectations. Sulphuretted hydrogen, which condenses readily at 3° F., under a pressure equal to that which balances the elastic force of an atmosphere compressed to $\frac{1}{14}$, had its elastic force increased so as to equal that of an atmosphere compressed to $\frac{1}{17}$ by an increase of 47° of temperature. Liquid muriatic acid at 3°, exerted an elastic force equivalent to that of an atmosphere compressed to $\frac{1}{20}$; by an increase of 22°, it gained an elastic

force equivalent to that of an atmosphere compressed to $\frac{1}{25}$; and by a farther addition of 26°, an elastic force equivalent to that of air condensed to $\frac{1}{40}$ of its primitive volume. These experiments were made in thick glass tubes hermetically sealed. The degree of pressure was estimated by the change of volume of air confined by mercury in a small graduated gage, and placed in a part of the tube exposed to the atmosphere, and the temperatures were diminished from the degree at which the gage was introduced, *i. e.* the atmospheric temperature by freezing mixtures; so that the temperature of the air within the gage could not be considerably altered; and as the elastic fluid surrounding the gage must have had a higher temperature than the condensed fluid, the diminution of the elastic force of the vapour from the fluids cannot be considered as overrated.

From the immense differences between the increase of elastic force in gases under high and low pressures, by similar increments of temperature, there can be no doubt that the denser the vapour, or the more difficult of condensation the gas, the greater will be its power under changes of temperature as a mechanical agent: thus carbonic acid will be much more powerful than muriatic acid. In the only experiment which has been tried upon it, its force was found to be nearly equal to that of air compressed to $\frac{1}{20}$ at 12° F., and of air compressed to $\frac{1}{36}$ at 32 degrees, making an increase equal to the weight of 13 atmospheres by an increase of 20 of temperature; and this immense elastic force of 36 atmospheres being exerted at the freezing point of water.*

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* Since this Paper was read, Mr. Faraday has ascertained that the vapour of ammonia at 32° exerts an elastic force equal to that of an atmosphere compressed

And azote, if it could be obtained fluid, would, there is no doubt, be far more powerful than carbonic acid; and hydrogen, in such a state, would exert a force almost incalculably great, and liable to immense changes from the slighest variations of temperature.

To illustrate this idea, I shall quote an experiment on alcohol of sulphur.

The temperature of this body was raised 20 degrees above its boiling point, and its elastie force examined: it was found equal to less than that of air compressed to $\frac{3}{4}$. It was now heated to 320° under a pressure equal to that of air condensed to $\frac{10}{77}$, and a similar increment of 20 degrees added: its elastic force became equivalent to that of an atmosphere compressed to $\frac{100}{895}$.

I hope soon to be able to repeat these experiments in a more minute and accurate way; but the general results appear so worthy the attention of practical mechanics, that I think it a duty to lose no time in bringing them forward even in their present imperfect state.

In applying the condensed gases as mechanical agents there will be some difficulty; the materials of the apparatus must be at least as strong and as perfectly joined as those used by Mr. Perkins in his high pressure steam engine: but the small differences of temperature required to produce an elastic force equal to the pressure of many atmospheres, will render the risk of explosion ex-

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to $\frac{1}{5}$; and at 50° to that of an atmosphere compressed to $\frac{10}{65}$: and that the vapour of nitrous oxide at 32° has an elastice force equal to that of an atmosphere compressed to $\frac{1}{44}$; and at 45° to an atmosphere compressed to $\frac{1.0}{51.3}$ nearly.

tremely small; and if future experiments should realise the views here developed, the mere difference of temperature between sunshine and shade, and air and water, or the effects of evaporation from a moist surface, will be sufficient to produce results, which have hitherto been obtained only by a great expenditure of fuel.

I shall conclude this communication by a few general observations arising out of this enquiry.

There is a simple mode of liquifying the gases, which at first view appears paradoxical, *namely, by the application of heat*; it consists in placing them in one leg of a bent sealed tube confined by mercury, and applying heat to ether, or alcohol, or water, in the other end. In this manner, by the pressure of the vapour of ether I have liquified prussic gas and sulphureous acid gas, the only two on which I have made experiments; and these gases in being reproduced occasioned cold.

There can be little doubt that these general facts of the condensation of the gases will have many practical applications. They offer easy methods of impregnating liquids with carbonic acid and other gases, without the necessity of common mechanical pressure.

They afford means of producing great diminutions of temperature, by the rapidity with which large quantities of liquids may be rendered aeriform; and as compression occasions similar effects to cold, in preventing the formation of elastic substances, there is great reason to believe that it may be successfully employed for the preservation of animal and vegetable substances for the purposes of food.

April 15, 1823.

APPENDIX TO THE PRECEDING PAPER.

On the changes of volume produced in gases in different states of density, by heat.

Read May 1, 1823.

In investigating the laws of the elastic forces exerted by vapours or gases raised from liquids by increase of temperature under compression, one of the most important circumstances to be considered is the rate of the expansion, or what is equivalent, of the elastic force, in atmospheres in different states of density.

It has been shown by the experiments of M. M. Dalton and Gay Lussac, that elastic fluids of very different specific gravities expand equally by equal increments of temperature, or, as it may be more correctly expressed, according to the elucidations of M. M. Dulong and Petit, that mercury and air, or gases, are equivalent in their expansions for any number of degrees in the thermometrical scale between the freezing and boiling points of water; and the early researches of M. Amontons seemed to show that the increase of the spring or elastic force of air by increase of temperature, was in the direct ratio of its density. I am not however acquainted with any direct researches upon the changes of volume produced in gases in very different states of condensation and rarefaction by changes of temperature, and the importance of the enquiry, in relation to the subject of my last communica-

tion to the Society, induced me to undertake the following experiments.

Dry atmospherical air was included in a tube by mercury, and its temperature raised from 32° Fahrenheit to 212°, and its expansion accurately marked. The same volumes of air, but of double and of more than triple the density under a pressure of 30 and 65 inches of mercury, were treated in the same manner, and in the same tubes; and when the necessary corrections were made for the difference of pressure of the removed column of mercury, it was found that the expansions were exactly the same.

An apparatus was constructed, in which the expansions of rare air confined by columns of mercury were examined and compared with the expansions of equal volumes of air under common pressure; when it appeared, that for an equal number of degrees of Fahrenheit's scale, and between 32° and 212° they were precisely equal, whether the air was $\frac{1}{2}$, $\frac{1}{3}$, or $\frac{1}{6}$ of its natural density.

Similar experiments were made, but they were necessarily less precise, with air condensed six and expanded fifteen times, with similar results.

XIX. *On the temperature at considerable depths of the Caribbean Sea. By Captain* EDWARD SABINE, *F.R.S., in a Letter addressed to Sir* HUMPHRY DAVY, *Bart. Pres. R.S.*

Read April 17, 1823.

MY DEAR SIR,

WHEN I last quitted England, you did me the favour to direct my attention to the temperature at considerable depths of fresh water lakes within the tropics, or of basins of salt water out of the reach of currents from the polar regions, and in climates where the lowest temperature of the superincumbent atmosphere is above 42°., as the point of the greatest density of water; viewing such knowledge in reference to the important question of the high temperature of the interior of the globe.

It has been a subject of continual regret to me, that situations in which the experiment could be tried, under circumstances which would alone render it decisive of the point in question, are of such rare occurrence, that not a single opportunity has presented itself to me on either side of the Atlantic, of carrying your suggestion into effect. As, however, it gave occasion to an attempt to ascertain the temperature of the Caribbean sea, at a depth beyond the means of sounding with which ships are usually provided, I may be permitted to take this mode of acquainting you of the result.

The Caribbean and Mexican seas, near the junction of which the experiment was made, approach the nearest, perhaps, to the character of insulated and deep basins of salt

water of any detached portions of the main ocean within the tropics; whilst however the connection is preserved, and by deep sea channels, it could scarcely be doubted, that if the colder water of distant regions finds its way to the equatorial parallels of the main ocean, it would no less pervade the corresponding depths in the same parallels, wheresoever a communication existed to admit its passage: if such an opinion required confirmation, it appears to have received it by the result of the present experiment.

A transcript of the memorandum which I wrote at the time, may probably be deemed the most satisfactory mode of relation.

"H. M. S. Pheasant, on passage between Grand Cayman Island and Cape St. Antonio, in Cuba, lat. 20½ N. lon. 83½ W., November 13th, 1822.

"At 2 P.M. hove too, and sounded with 1230 fathoms of line, being 11 coils of 113 fathoms each, and three fathoms of a 12th coil; at the end of the line was attached a strong iron cylinder of 75 lbs. weight, enclosing a Six's self-registering thermometer: the top of the cylinder screwed down upon leather, being designed, by excluding the water from the interior, to obviate any effect which might be supposed to arise from the increased pressure of water at great depths: the thermometer fitted into spiral springs at the top and bottom, which kept it from contact with other parts of the cylinder, and preserved it from injury, in case the apparatus should accidentally strike against the sides of the ship, or against rocks at the bottom: another iron cylinder, of much less strength and weight than the preceding, was attached two fathoms above the end of the line, and being pierced

with holes in the top and bottom, admitted free access of the water to a second thermometer, of similar construction to the first. The opportunity was very favourable for the object, the weather being fine, with light airs and but little swell: the 1230 fathoms run out in rather more than 25 minutes, at the expiration of which time the line was fairly on the quarter, the ship's drift having been bodily to leeward, without her having had either head or stern way; there was consequently much less stray line than had been anticipated. The best practical judgment which Captain Clavering could form on the spot was, that the depth to which the thermometers had actually attained must have exceeded a thousand fathoms, as an allowance of the remaining 230 fathoms for stray line would certainly be more than ample, if no bight of consequence existed in the rope, which, from the appearance, and from the rapidity with which the weight drew out the line, might be judged the case: 230 fathoms would equal a drift to leeward of $\frac{4}{5}$ths of a mile in 25 minutes, whereas that of the ship did not exceed $\frac{1}{2}$ a mile an hour; it is more than probable, therefore, that the depth is underrated when it is estimated at 1000 fathoms, or 6000 feet. The line was hauled in in 53 minutes, and the thermometers came up in good order; the one in the cylinder to which the water had free access had registered 45°,5; the attempt to exclude the water from the other cylinder did not in this instance altogether succeed, in consequence of the top not having been screwed down sufficiently close upon the leather; this thermometer had registered 49°,5; the difference of 4° may be attributed, perhaps, partly to the latter not having been so long in contact with the cold water as the other thermometer,

as the water appeared to have had great difficulty, and was probably some time in forcing its way into the interior of the closed cylinder; and partly to the heat which so great a thickness of metal would retain for a considerable time; the surface water was from 82°,5 to 83°,2 in the course of the afternoon; the difference of temperature between the surface, and a depth exceeding 1000 fathoms was, therefore, 33°,3 by one thermometer, and 37°,3 by the other, the indication of the latter being entitled to the most reliance.

"It may be reasonably inferred, that one or two hundred fathoms more line would have caused the thermometer to have descended into water at its maximum of density, as depends on heat, below which, consequently, no farther diminution of temperature would take place; this inference being on the presumption that the greatest density of salt water occurs, as is the case in fresh water, at several degrees above its freezing point.

"It is desirable to repeat this experiment, should an opportunity present itself, and to attach several register thermometers, at intervals of the line, to ascertain the progression in which the temperature diminishes; and it would be still more interesting to determine the depth beneath the surface at which water is found, at the term of the diminution of its temperature in different parallels."

The opportunity which was thus hoped for, did not occur in the remainder of the voyage; the experiment requires favourable circumstances of weather, and of sea, to both of which the season was adverse.

I am not aware of any previous experiments on the temperature of the deep sea within the tropics, except those

of Monsieur Perron, the imperfection of whose apparatus prevented his obtaining equally decisive results. Being unprovided with a registering thermometer, he enclosed one of the ordinary construction in several cases filled with non-conducting substances, in order that the temperature which the thermometer had acquired, by being suffered to remain a sufficient time below, might not be disturbed by its being drawn up through the warmer strata. It appears, however, that in his principal experiment, in which the apparatus was sunk to 2144 feet, the process of raising it lasted three-quarters of an hour, or half the time it had remained below: the difference therefore between the surface and the deep sea water, may be supposed to have been actually greater than would appear in his results; they are in accord however with the present, in showing even a more rapid decrease of temperature in descending from the surface.

In lat. 5° North, 38° degrees of Fahrenheit less than at the surface at 1200 feet; and in lat. 4° North, 42 degrees of Fahrenheit less than at the surface at 2144 feet.

The thermometers used in the present Experiment were made expressly for the purpose to which they were applied, and were of the ordinary construction of Six's registering thermometer: the top of the tube, in which is contained the index of heat, was hermetically sealed instead of being closed by a cork, as is sometimes the case. I have no reason to doubt the accuracy of their performance in any respect.

I remain, my dear Sir
your obliged and faithful Servant,
EDWARD SABINE.

2, Portland Place,
March 18, 1823.

XX. *Letter from Captain* BASIL HALL, *R. N. to Captain* KATER, *communicating the details of experiments made by him and Mr.* HENRY FOSTER, *with an Invariable Pendulum; in London; at the Galapagos Islands in the Pacific Ocean, near the Equator; at San Blas de California on the N. W. Coast of Mexico; and at Rio de Janeiro in Brazil. With an Appendix, containing the Second Series of Experiments in London, on the Return.*

Read April 24, 1823.

His Majesty's Ship Conway,
Spithead, 23rd February, 1823.

MY DEAR SIR,

I HEREWITH transmit the details of the experiments which have been made with the invariable pendulum, placed in my hands by the Board of Longitude, at your suggestion.

It is matter of regret to me, that I should have visited so many remote places, with such means in my hands, and have so few results to produce. The fact however is, that the service upon which I was sent had no connection with scientific research, and that it was only at casual intervals of active professional employment, that I had any leisure for enquiries of this nature. These occasional opportunies I owe to the generous indulgence of Sir THOMAS HARDY, the Commander in Chief, to whose assistance, also, and encouragement in every pursuit having useful knowledge for its object, I stand essentially indebted.

In drawing up the following account, care has been taken to state all the attendant circumstances, and to record in the Tables every observation in detail; so that any person wishing to examine the work, may have the best means possible of estimating its value.

The methods followed for making the adjustments of the instruments, conducting the experiments, and deducing the results, were those laid down in your paper on the length of the pendulum at the principal stations of the Trigonometrical Survey. I took care, for example, always to adjust the diaphragm in the focus of the eye-piece of the telescope, so that its edges should coincide exactly with those of the extremity of the pendulum; according to the precept at page 9 of your second paper. (read before the Royal Society in June, 1819.) This adjustment, by the way, is rendered more easy and exact, by placing a card or other white object at a little distance behind the pendulum. I also invariably determined the Intervals by observing the disappearance of the white disk according to your directions at page 11, and the reasoning at page 58 of the first paper,* (read in January, 1818.)

And here I feel it not only due to you, but likely, perhaps, to be of use to future observers, to state that, after many trials of fancied improvements, and simplifications of your methods, both in the conduct of the experiment itself, and in the subsequent computations, I was finally obliged to acknowledge, in every instance, even where I succeeded, that I had by more labour, or by a more circuitous path, reached the same point to which your rules would at once have led me.

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* I am particular in stating these two circumstances, especially the first, from its being so essential to the accuracy of the whole experiment, in all cases where the diameter of the disk and the breadth of the pendulum, though in fact equal, are placed at different distances from the eye, and therefore appear under different angles; and not, as in your first paper, where they are so proportioned that both occupy the same apparent space when seen through the telescope. I was at first disposed to think it might be better to observe both the times of disappearance and re-appearance of the white disk, and to assume the mean as the true instant of the coincidence; but I found by repeated trials, that the time of re-appearance was liable to greater or less uncertainty according to the degree of light, and other unmanageable circumstances: and having satisfied myself by demonstration that the method of obtaining the intervals by observing the disappearance was rigorously correct in principle, I adhered to it ever afterwards as being more simple and infallible in practice.

From having carefully studied your works before leaving England, I had conceived myself to be sufficiently qualified to undertake a course of experiments at once. In this, however, I was mistaken; and the consequence has been, that of two extensive series which I made at Valparaiso, neither is I fear sufficiently accurate to deserve your notice. The experience, however, which I gained in the course of these operations, enabled me ever afterwards to proceed with confidence. And here I may take occasion to suggest the advantage which, on future occasions, would arise from having the whole experiment performed in England, by the person who is afterwards to repeat it abroad, not under the hospitable roof of Mr. Browne, to whose valuable assistance every one who has attended to this subject, is so deeply obliged, but in the fields, and with no advantages save those which he could carry with him. He would thus in good time discover omissions in his apparatus, which are not to be supplied abroad, and be aided in surmounting difficulties before he had sailed, as I did, beyond the reach of appeal.

The first series of experiments, No. I. was made, as you know, in London. The next, which is marked No. II, was made thirty two miles and a half north of the equator, at one of the Galapagos, a group of islands in the Pacific, lying upwards of two hundred leagues west from the Continent of South America. It was intended that a station should have been chosen immediately under the line, but the ship being swept to leeward in the course of the night by a strong current, this object could not be effected without losing more time than circumstances admitted of being spent in that quarter.

The spot chosen for the experiments lies near the extremity

of a point of land running into the sea at the south end of Abingdon Island, where it forms the western side of a small bay, about a mile across. The point is part of an ancient stream of lava which has flowed down the side of a peaked mountain in the middle of this end of the island. The summit of this peak is between two and three miles from the station, in a direction nearly north, and is about two thousand feet high: it slopes rapidly at first, forming a tolerably steep cone, but terminated by a broad and gently sloping base of a mile and a half. The sides of the mountain are studded with craters, or mouths, from whence at different periods streams of lava have issued, and running down to the sea, have there formed projecting points, such as that on which we fixed our station. The western face of the island presents a cliff nearly perpendicular, and not less than a thousand feet high; it exhibits the rude stratification of lava, tuffa, and ashes, which characterizes the fracture of ancient volcanic mountains. I am thus minute in describing this island, that you may be enabled to judge how far its density may have modified the results of the experiments. It is ten or twelve miles long; the north end being a continued system of long, low, and very rugged streams of lava; the peak standing about one-third of the whole length from the southern extreme, where our station was. The rock at different places not far from the station was found to be full of caves, into which the tide flowed and ebbed through subterranean channels; the outer crust of the stream having, as usual, served as a pipe to conduct the lava off: it is therefore probable that our foundation may not have been the solid rock, a circumstance which, taken along with the general hollow nature of volcanic districts, and the deepness of the surrounding ocean, renders these experiments

not so fit to be compared with those made in England, as with others made on a similar volcanic soil.

The range in the temperature in 24 hours was from 74° to 91°, and as we were obliged to place the instruments in a tent, the temperature rose in the day time, and fell at night, but without any uniformity. On the first day of observing coincidences, a set was taken after breakfast, and another before dinner; but as it was soon seen that this would be to confine the whole of the observations to the hot period of the day, it was determined in future to take one set as soon after sun-rise as possible, in order to have a result which should be influenced by the whole night's continued low temperature; and another set towards the close of the day, in order to have a result partaking in like manner of the influence which the whole day's high temperature might have on the length of the pendulum. I also endeavoured so to arrange things, that I should catch a sufficiently long period of uniform temperature during the interval of each set, that it might be taken with an unvarying thermometer; hoping that by these arrangements, although no one experiment could produce strictly correct results, the opposite errors of the morning and evening observations would counterbalance one another; that is, that the mean, between observations taken both in the hot and in the cold periods of the day, would probably give a just result; or at least such a result as would fairly be entitled to stand by the side of rates deduced from transits of stars, the intervals between taking which, in like manner, included the same extremes of temperature.

It should be borne in mind that the real desideratum, as far as respects rate, is not to know what is the aggregate loss or gain of the clock in twenty-four hours, but the actual rate at

which the clock is going during the particular period of observing; or, in other words, that number of beats, and parts of a beat which, were the clock to go on uniformly from that period, would be indicated by its dial plate, in twenty-four hours, or 86400 seconds of mean time. As the method of transits of stars, however, gives no more than average rates, I sought, by the arrangements above stated, to obtain, in like manner, average results from the mean of observations made at the extreme temperatures.

A thermometer was suspended so that its bulb stood one inch in front of the middle part of the pendulum, and another was hung between the clock case and the pendulum, lower down. The average temperature at night was 74°, and in the day time 86° and 90°; the latter, as I have said, depending principally on the state of the sky. The allowance for expansion was made from the deductions which resulted from your experiments on a similar pendulum; but I propose instituting a series of experiments with the pendulum which I used, in order to investigate this important branch of the subject more directly.

An astronomical circle, by TROUGHTON, was used as a transit instrument, and was so placed in a small octagonal observatory of light pannels communicating by a door with the tent, that the clock could be seen, and its beats heard by the observer at the instrument; thus, with the exception of the first day's transits, the time was recorded directly from the clock, without the intervention of a chronometer. The meridian mark was placed near the sea, at the distance of 806 feet: a strong post having been driven into a cleft of the rock and firmly secured, there was nailed to it a screen made of copper, and perforated with a system of holes from one-fourth to one

tenth of an inch in diameter, and readily distinguishable from the Observatory. The screen being, moreover, made in the form of a box to receive the lamp, it became impossible to misplace the light. The instrument was brought down to this mark, and the level carefully examined before and after every observation, except with some stars which followed too close upon one another. The sun was fortunately observed at noon every day; and as its rays were never allowed to touch any part of the instrument, or to enter the Observatory, except at the moment of noon, and then only through a small hole, I had reason to hope that none of the adjustments were at this observation ever deranged. As the great alternations in temperature alluded to above, might naturally be expected to cause fluctuations in the going of the clock, it was satisfactory to have a series of regularly, and frequently recurring tests, brought to bear upon this essential particular. As the same precautions were observed at every station, this account of them will apply to the whole series of experiments.

But in order to your forming no higher than a correct estimate of this insulated experiment, it is right I should describe to you the peculiar circumstances under which it was performed. It was above all to be regretted that we were so much limited in time, that we could not engage in a fresh series, either at the same island, or on some other lying nearer the equator, the service upon which the Conway was employed, rendering it necessary that our stay should not be longer at the Galapagos than the 16th of January. Now, as we anchored at Abingdon's Island on the 7th at noon, there were barely nine complete days in which every thing was to be done. We had to search for a landing place, which occu-

pied some considerable time; to decide upon a station; our tents to rig up; the Observatory to build; then to land the instruments, and set them up; and as we had no time for trials and alterations, every thing required to be permanently fixed at once. We were fortunate in weather during the first two days, when our things were all lying about, and our habitations ill assorted; but on the third night it rained hard, and the water which trickled through the canvas caused us some discomfort, although we fortunately succeeded in sheltering the instruments. The heat during the day was not only opppressive at the time, but very exhausting in its effects; and at night, although the thermometer never fell lower than 73°, the feeling of cold arising from the transition from 93°, to which it sometimes rose in the day, was no less disagreeable.

It was with reluctance that I left the neighbourhood of the equator, without having made more numerous and more varied, and consequently more unexceptionable observations on the length of the pendulum. It would, above all, have been desirable to have swung it at stations more nearly resembling those with which its vibrations were to be compared. Thus, the results obtained from the experiments at the Galapagos, though curious in themselves, are not so valuable for comparing with those you have deduced in this country. The time may come, however, when they may be rendered more useful; that is to say, should experiments be made with the same pendulum at stations remote from the Galapagos, but resembling them in insular situation, in size, and in geological character; such as the Azores, the Canaries, St. Helena, the Isle of France, and various stations amongst the eastern islands of the Indian and the Pacific oceans. The advantage of having it swung at the Cape of Good Hope,

and especially at the Falkland Islands (which lie in the correspondent latitude to that of London), and at various other stations on the main land, or on large islands, is still more obvious.

At page 240 you will observe the details of the ellipticities deduced; and it is sufficient to mention here, that the length of the second's pendulum at the Galapagos is 39.01717 inches, and the mean of all the ellipticities thereby deduced from your experiments in England, $\frac{1}{284.98}$, and from those of Captain Sabine at Melville Island, $\frac{1}{292.14}$.

SAN BLAS DE CALIFORNIA.*

The tables No. III. contain the details of the experiments made at San Blas, a sea port town on N.W. coast of Mexico, in latitude $21\frac{1}{2}°$ N. and longitude $105\frac{1}{4}°$ W. and not far from the south point of California. These experiments were performed under favourable circumstances, the sky being clear, the temperature steady, and the rate of the clock uniform. The station indeed was more elevated than could have been wished, being 115 feet above the level of the sea, on the summit of a cylindrical rock of compact whin stone, and measuring not more than 500 feet across, and nearly perpendicular in three quarters of its circumference.

The length of the seconds pendulum at San Blas, by these experiments, comes out 39.03776 inches, and the mean ellipticity $\frac{1}{313.55}$.

By a second series of experiments at San Blas, the details of which are given by my coadjutor, Mr. Henry Foster, Mas-

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* San Blas is in Mexico, but being near California, it takes that addition to distinguish it from other towns of the same name.

ter's Mate of the Conway, in Experiment No. IV, the length of the seconds pendulum comes out 39.03881, and the mean ellipticity $\frac{1}{308.56}$. The circumstances in this case, however, were not so favourable as those of the first series, being to one another in the ratio of 47 to 397, or nearly as 1 to 8. This arose from the change which took place in the weather at that period, the sky being overcast, the temperature fluctuating, and the rate of the clock unsteady.

RIO DE JANEIRO.

Two extensive series of experiments were made at this place, first by myself, No. V. and then by Mr. FOSTER, No. VI. The total number of the factors in the first case being 210, and in the second 452. The results agree with surprising exactness for operations entirely unconnected. The length of the seconds pendulum by my experiments being - - 39.04381

By Mr. FOSTER - - - - - 30.04368

The mean ellipticity by my experiments is - - $\frac{1}{301.77}$

By Mr. FOSTER - - - - - - $\frac{1}{302.37}$

The circumstances in both cases were favourable, especially in the steadiness of the temperature, and the uniformity of the clock's rate; but as they were decidedly most favourable in the case of Mr. FOSTER's experiments, I have no hesitation in considering his as the most entitled to credit.

Mr. FOSTER is the Gentleman to whose co-operation I owed so much when observing the comet at Valparaiso; an account of which, in a letter to Dr. WOLLASTON, has appeared in a recent Volume of the Transactions. His present work speaks sufficiently for itself; but I should be doing him scanty justice by confining myself to such a reference, without also

stating that, occupied as I was with professional duties, it would have been hopeless to have undertaken these experiments, without the zealous assistance of a person who, besides being free to attend exclusively to the subject, was thoroughly skilled in all its details.

I remain, my dear Sir,
most sincerely yours,
BASIL HALL.

APPENDIX.

Being desirous of presenting an account of these operations to the Royal Society before the vacation, I had not time to repeat the experiments in London before the above letter was read. Since that period, however, I have ascertained, by careful observations, that the number of vibrations made by the pendulum now, does not accord with that which resulted from the experiments made in London before the voyage. By a reference to the additional Tables in the Appendix, page 285* to 288,* and the Remarks which follow, the amount of this discordance will be seen, as well as the explanation of the cause. It is only necessary to mention here that the ellipticities, printed in the above letter, have all been recomputed on the principle stated in the remarks alluded to.

Abstract of the most exact results at each station.

Stations.	Diminution of Gravity from Pole to Equator.	Ellipticity.	Length of Equat. Pend.
Galapagos, 0° 32′ ″ N	,0051412	$\frac{1}{284,98}$	39,017196
San Blas, . 21 30 25 N	,0054611	$\frac{1}{313,55}$	39,00904
Rio, . . 22 55 22 S	,0053431	$\frac{1}{302,37}$	39,01206

B. H.

London, 30*th August,* 1823.

Experiment No. I. at London.

Observation of Coincidences.

3d July, 1820, P.M. at Mr. Browne's. Lat. 51° 31′ 8″, 4 } Bar. 29,92.
Height above the sea 83 feet. Clock losing 1^s,60 }

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.		°					
67,5	0 4 17	1,19						
			1,13	1014			2,09	
	21 11	1,07						
			1,00	1015			1,64	
	38 6	0,94						
			0,90	1019			1,33	
	55 5	0,86						
			0,82	1020			1,23	
	1 12 5	0,78						
			0,74	1021			0,90	
68,2	29 6	0,71						
67,8	Mean			1017,8	1015,8	86228,63	1,44	86230,07

4th July, P. M. at Mr. Browne's. } Bar. 30,0.
Clock losing - - - 1^s,40 }

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
	h. m. s.							
66,9	0 54 41	1,20						
			1,14	1010			2,13	
	1 11 31	1,08						
			1,02	1013			1,71	
	28 24	0,97						
			0,92	1015			1,39	
	45 19	0,88						
			0,84	1018			1,16	
	2 2 17	0,80						
			0,76	1016			0,95	
	19 13	0,73						
			0,69	1020			0,78	
68,1	36 13	0,65						
67,5	Mean			1015,33	1013,33	86228,41	1,35	86229,76

5th July, P. M. at Mr. Browne's.
Clock losing - - - $1^s,20$ } Barom. 30,10.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
	h. m. s.							
66°,0	0 11 13	1,15						
			1°,09	1016			1,95	
	28 09	1,04						
			0,99	1018			1,60	
	45 7	0,94						
			0,89	1019			1,30	
	1 2 6	0,85						
			0,81	1019			1,08	
	19 5	0,77						
			0,74	1022			0,94	
	36 7	0,70						
			0,66	1023			0,71	
66,6	53 10	0,63						
66,3	Mean			1019,5	1017,5	86229,31	1,26	86230,57

6th July, P. M. at Mr. Browne's.
Clock losing - - - $1^s,10$ } Barom. 30,13.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
	h. m. s.							
65,6	1 16 39	1,18						
			1,12	1014			2,06	
	33 33	1,07						
			1,01	1016			1,68	
	50 29	0,96						
			0,91	1017			1,36	
	2 7 26	0,87						
			0,83	1019			1,13	
	24 25	0,79						
			0,76	1018			0,94	
	41 23	0,73						
			0,70	1021			0,80	
66,7	58 24	0,67						
66,1	Mean			1017,5	1015,5	86229,07	1,33	86230,40

7th July, P. M. at Mr. Browne's.
Clock losing - - - 1^s,15 } Barom. 30,13.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
	h. m. s.							
65°,8	0 30 43	1,16						
			1°,10	1016			1,98	
	47 39	1,05						
			1,00	1016			1,64	
	1 4 35	0,95						
			0,91	1019			1,36	
	21 33	0,87						
			0,83	1021			1,13	
66,3	38 35	0,79						
66,0	Mean			1018	1016	86229,11	1,53	86230,64

8th July, P. M. at Mr. Browne's.
Clock losing - - - 1^s,00 } Barom. 30,15.

	h. m. s.							
65,7	11 24 49	1,27						
			1,21	1011			2,40	
	41 40	1,15						
			1,09	1013			1,95	
	58 33	1,04						
			0,99	1013			1,60	
	0 15 26	0,94						
			0,90	1017			1,33	
	32 23	0,86						
			0,82	1018			1,10	
	49 22	0,77						
			0,73	1019			0,88	
66,5	1 6 20	0,70						
66,1	Mean			1015,16	1013,16	86228,79	1,54	86230,34

9th July, P. M. at Mr. Browne's.
Clock losing - - - 0,90 } Barom. 30,15.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°						
65,1	11 37 4	1,19						
			° 1,13	1017			2,09	
	54 1	1,07						
			1,02	1016			1,71	
	12 10 57	0,98						
			0,94	1019			1,45	
	27 56	0,88						
			0,84	1018			1,15	
	44 54	0,80						
			0,76	1021			0,95	
66,3	1 1 55	0,72						
65,7	Mean			1018,2	1016,2	86229,39	1,49	86230,88

Results.

Date.	Barom.	Therm.	Vibrations in 24 hours.	Correction for Temperature.	Vibrations at 68 degrees.
July 3	29,92	° 67,8	86230,07	— 0,08	86229,99
4	30,00	67,5	86229,76	— 0,21	86229,55
5	30,10	66,3	86230,57	— 0,72	86229,85
6	30,13	66,1	86230,40	— 0,70	86229,70
7	30,13	66,0	86230,64	— 0,85	86229,79
8	30,15	66,1	86230,34	— 0,70	86229,64
9	30,15	65,7	86230,88	— 0,97	86229,91
Mean	30,08	66,5			86229,78
			Correction for Buoyancy		+ 5,98
					86235,76
			Correction for 88 feet		+ 22
No. of vibrations at London in vacuo, at the level of the sea, in temperature 68° . .					= 86235,98

Experiment No. II. at the Galapagos.

Transits observed at the Earl of Abingdon's Island.

Date.	Star.	1st Wire.	2nd. Wire.	Mer. Wire.	4th Wire.	5th Wire.	Mean Chron.	Clock.
January, 1822.		h. m. s.	m. s.	m. s.	m. s.	m. s.		
9	⊙'s 1st Limb	12 14 47, 5	15 16, 2	15 44 ,0	16 10, 2	16 38, 0		
	⊙'s 2d Limb	0 17 8, 8	17 36, 8	18 4, 5	18 30, 8	18 59, 0		
	⊙'s Centre	12 58 58,15	16 26,50	16 54,25	17 20,50	17 48,50	Centre Clock. 12 16 53,62	Clock at mean Noon, 12 9 23,82
							Chro.	Clock.
P. M.	12 Eridani	11 33 32, 2	34 2, 0	34 31, 5	34 59, 5	35 29, 0	11 34 30,95	7 57 29,28
	ι Eridani	. . . .	11 54 6, 5	54 32, 0	54 57, 0	. . .	11 54 31,87	8 17 29,57
	δ Eridani	12 3 48, 5	4 14, 7	4 40, 5	5 5, 2	5 31, 5	12 4 40,15	8 27 37,50
	Rigel	9 57 43, 5	58 9, 0	58 35, 4	59 1, 2	59 29, 5	Obsd. by Clock.	9 58 35,50
	η Orionis	1 44 20, 7	44 46, 7	45 12, 5	45 36, 7	46 1, 7	1 45 11,80	10 8 5,80
	δ Orionis	1 51 43, 0	52 8, 7	52 34, 7	52 58, 7	53 24, 7	1 52 34,08	10 15 27,85
	ι Orionis	1 55 58, 2	56 24, 2	56 50, 0	57 14, 1	57 40, 0	1 56 49,67	10 19 43,37
	ζ Orionis	2 0 33, 0	00 59, 5	1 24, 7	1 49, 5	2 14, 7	2 1 24,35	10 24 17,68
	κ Orionis	2 8 2, 7	8 29, 0	8 55, 2	9 20, 0	9 46, 1	2 8 54,70	10 31 47,70
		After this day the Transits were observed directly with the Clock.						
10	⊙'s 1st Limb	12 14 13, 0	14 40, 0	15 6, 8	15 34, 2	16 2, 0		
	⊙'s 2d Limb	0 16 32, 8	17 0, 8	17 28, 5	17 55, 0	18 22, 8		
	⊙'s Centre	12 15 22,90	15 50 40	16 17,65	16 44,60	17 12,40	Centre 12 16 17,57	Clock at mean Noon. 12 8 23,27
								Mean Clock.
10 P. M.	ζ Eridani	7 54 24, 8	54 50, 8	55 16, 4	55 41, 2	56 7, 0	—	7 55 16,10
	ε Eridani	8 11 42, 0	12 8, 2	12 34, 2	12 58, 5	13 24, 5	—	8 12 33,60
	δ Eridani	8 21 50, 2	22 16, 5	22 43, 5	23 7, 5	23 33, 5	—	8 22 42,45
	β Eridani	9 45 58, 8	46 24, 0	46 49, 0	47 13, 8	47 40, 0	—	9 46 49,10
	Rigel	9 52 48, 5	53 14, 5	53 40, 2	54 5, 0	54 31, 5	—	9 53 39,98
	η Orionis	. . . .	10 2 46, 0	3 11, 5	3 36, 0	4 2, 0	—	10 3 11,25
	δ Orionis	10 9 41, 8	10 8, 0	. . .	10 59, 0	. . .	—	10 10 33,50
	ι Orionis	10 13 56, 8	14 23, 0	14 49, 0	15 13, 0	15 39, 0	—	10 14 48,30
	ζ Orionis	10 18 32, 0	18 57, 5	19 23, 5	19 48, 0	20 13, 8	—	10 19 23,05
11	⊙'s 1st Limb	12 13 37, 0	14 5, 2	14 32, 0	14 58, 0	15 25, 0		
	⊙'s 2d Limb	0 15 57, 2	16 25, 2	16 52, 8	17 19, 2	17 47, 1		
	⊙'s Centre	12 14 47,10	15 15,20	15 42,40	16 8,60	16 36,05	Centre. 12 15 41,96	Clock at mean Noon. 12 7 23,76
11 P. M.	Evening Cloudy.							
12	⊙'s 1st Limb	12 13 3, 2	13 32, 0	13 59, 0	14 24, 2	14 52,5		
	⊙'s 2d Limb	0 15 23, 5	15 51, 2	16 18, 5	16 45, 0	17 13, 0		
	⊙'s Centre	12 14 13,35	14 41,60	15 8,75	15 34,60	16 2,75	12 15 8,30	12 6 26,90
12 P. M.	Evening Cloudy							
13	⊙'s 1st Limb	12 12 27, 2	12 55, 2	13 23, 0	13 49, 2	14 17, 0		
	⊙'s 2d Limb	0 14 47, 5	15 15, 8	15 43, 8	16 10, c	16 37, 4		
	⊙'s Centre	12 13 37,35	14 5,50	14 33,40	14 59,60	15 27,20	12 14 32,77	12 5 28,67

Transits continued.

Date.	Star.	1st Wire.	2nd Wire.	Mer. Wire.	4th Wire.	5th Wire.	Mean Clock.	Clock at Mean Noon.
Jan. 1822.		h. m. s.	m. s.	m. s.	m. s.	m. s.	h. m. s.	h. m. s.
13 P. M.	α Reticuli	8 43 20, 5	44 18, 0	45 14, 0	46 7, 0	47 3, 8	8 45 12,88	
	β Eridani	9 31 15, 0	31 40, 5	32 6, 0	32 31, 0	32 57, 0	9 32 5,92	
	Rigel	9 38 5, 2	38 31, 0	38 58, 0	39 23, 0	39 49, 0	9 38 57,37	
	η Orionis	9 47 37, 0	48 4, 0	48 29, 0	48 54, 2	49 20, 0	9 48 28,87	
	δ Orionis	9 54 59, 5	55 25, 2	55 51, 0	56 15, 5	56 41, 0	9 55 50,53	
	ε Orionis	9 59 14, 2	59 40, 0	0 6, 0	0 30, 0	0 56, 0	10 0 5,37	
	ζ Orionis	10 3 49, 2	4 15, 2	4 41, 0	5 5, 0	5 31, 0	10 4 40,40	
14	☉'s 1st Limb	12 11 48, 0	12 18, 0	12 45, 5	13 12, 2	13 39, 5		
	☉'s 2d Limb	14 10, 0	14 37, 8	15 5, 5	15 31, 2	15 59, 5		
	☉'s Centre.	12 12 59, 0	13 27,90	13 55,50	14 21,70	14 49,50	12 13 54,85	12 4 28,65
14 P. M.	12 Eridani	7 31 56, 5	32 26, 5	32 56, 0	33 24, 0	33 54, 0	7 32 55,50	
	ζ Eridani	7 34 45, 2	35 12, 0	35 37, 0	36 2, 0	36 28, 5	7 35 36,95	
	ε Eridani	7 52 3, 0	52 29, 0	32 55, 5	53 20, 0	53 47, 0	7 52 55,00	
	ο Eridani	8 2 11, 5	2 38, 0	3 4, 0	3 29, 0	3 55, 0	8 3 3,58	
	α Reticuli	8 38 23, 8	39 21, 5	40 18, 0	41 11, 0	42 8, 5	8 40 16,80	
	β Eridani	. . . .	26 44, 0	27 10, 0	27 34, 0	28 5, 0	9 27 9,50	
	Rigel	9 33 9, 0	33 35, 0	34 1, 8	34 26, 0	34 52, 5	9 34 1,02	
	η Orionis	9 42 40, 5	43 6, 2	43 32, 0	43 56, 2	44 22, 5	9 43 31,57	
	δ Orionis	9 50 2, 5	50 28, 2	50 54, 8	51 18, 2	51 44, 0	9 50 53,75	
	ε Orionis	9 54 17, 2	54 43, 0	55 8, 2	55 33, 0	55 59, 5	9 55 8,18	
	ζ Orionis	9 58 52, 2	. . .	59 43, 2	0 8, 0	0 34, 2	9 59 43,20	
	κ Orionis	10 6 21, 5	6 47, 2	7 13, 5	7 38, 0	8 4, 8	10 7 13,08	
15	☉'s 1st Limb	12 11 10, 0	11 38, 2	12 5, 5	12 31, 5	12 59, 2		
	☉'s 2d limb	13 30, 0	13 57, 8	14 25, 2	14 51, 5	15 20, 0		
	☉'s Centre	12 12 20,00	12 48,00	13 15,35	13 41,50	14 9,60	12 13 14,97	12 3 27,27
15 P. M.	ε Eridani	7 47 6, 8	47 33, 0	47 59, 0	48 24, 0	48 50, 0	7 47 58,63	
	δ Eridani	7 57 15, 0	57 41, 0	58 7, 8	58 32, 0	58 58, 5	7 58 7,02	
	ζ Orionis	. . . .	. . .	, . .	. . .	9 55 37, 5	9 55 37,50	5th Wire.

For the Rates deduced from these Transits, see Tables III. and IV.

Comparisons of Clock with Chronometer.

Date.	Chronometer.	Clock.	Clock Fast.
January 1822.	h. m. s.	h. m. s.	h. m. s.
9 P.M.	11 42 0	20 4 58,0	8 22 58,0
——	0 00 0	8 22 57,5	57,5
——	0 8 0	8 30 57,2	57,2
——	1 43 0	10 5 54,0	54,0
——	2 11 0	10 33 53,0	53,0

A. M. 10th January, 1822, Galapagos. } Barom 30,025.
Clock losing $59^s,36$ at a mean rate.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of Clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
81, 2	9 26 56	1,25						
			1,20	683			2,35	
83, 0	38 19	1,16						
			1,13	682			2,09	
83, 8	49 41	1,09						
			1,05	684			1,80	
84, 1	10 01 5	1,01						
			0,97	683			1,54	
85, 2	12 28	0,93						
			0,89	685			1,29	
85, 8	23 53	0,86						
			0,83	683			1,12	
87, 8	35 16	0,80						
			0,78	683			0,99	
87, 1	46 39	0,75						
			0,72	683			0,85	
87, 8	58 2	0,70						
85,09	10 4 Mean Solar Time			683,25	681,25	86087,91	1,50	86089,41

P. M. 10th January. Barom. 29,90.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of Clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
	h. m. s.							
91, 2	1 53 23	1,30						
			1,25	674			2,55	
90, 1	2 4 37	1,21						
			1,17	676			2,24	
89, 8	15 53	1,13						
			1,09	676			1,94	
90, 2	27 09	1,05						
			1,02	676			1,70	
91, 3	38 25	0,98						
			0,95	677			1,48	
90, 1	49 42	0,92						
			0,89	677			1,29	
88, 0	3 0 59	0,86						
			0,83	679			1,12	
87, 2	12 18	0,80						
			0,77	677			0,97	
87, 0	23 35	0,75						
89,43	2 30 Mean Solar Time.			676, 5	674,5	86085,38	1,66	86087,04

A. M. 11th January. Barom. 29,965.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of Clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°						
73,3	6 44 12	1,23						
			° 1,17	690			2,24	
74,2	55 42	1,12						
			1,08	689			1,91	
75,0	7 7 11	1,03						
			0,99	689			1,60	
75,5	18 40	0,95						
			0,91	689			1,35	
—	30 9	0,88						
			0,84	688			1,15	
77,0	41 37	0,80						
			0,77	687			0,97	
78,0	53 4	0,73						
			0,69	688			0,78	
78,5	8 4 32	0,65						
			0,61	686			0,61	
79,5	15 58	0,56						
76,37	7 23 Mean Solar Time			688,25	686,25	86089,75	1,33	86091,08

P. M. 11th January. Barom. 29,90.

	h. m. s.							
84,5	5 1 51	1,32						
			1,27	680			2,64	
—	13 11	1,22						
			1,18	680			2,28	
83,5	24 31	1,14						
			1,10	678			1,98	
83,0	35 49	1,07						
			1,04	680			1,77	
82,5	47 9	1,00						
			0,96	680			1,51	
82,0	58 29	0,93						
			0,90	681			1,32	
81,8	6 9 50	0,87						
			0,84	682			1,15	
81,0	21 12	0,81						
82,61	5 30 Mean Solar Time			680,14	678,14	86086,75	1,81	86088,56

A. M. 12th January, 1822, Galapagos.
Clock losing 59^s,36 at a mean rate.
Barom. 29,93.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of Clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
74,5	6 20 43	1,30						
			1,25	679			2,25	
74,2	32 02	1,20						
			1,15	678			2,16	
74,2	43 20	1,14						
			1,07	678			1,87	
74,2	54 38	1,04						
			1,01	682			1,67	
74,5	7 6 00	0,97						
			0,93	682			1,41	
74,7	17 22	0,90						
			0,87	684			1,24	
75,0	28 46	0,84						
			0,81	682			1,07	
76,2	40 8	0,78						
			0,75	682			0,92	
77,5	51 30	0,72						
75,0	7 0 Mean Solar Time			680,875	678,87	86087,03	1,61	86088,64
P. M. 12th January.						Barom. 29,90.		
°	h. m. s.							
84, 0	4 20 51	1,30						
			1,25	678			2,55	
83, 5	32 9	1,20						
			1,15	680			2,16	
83, 1	43 29	1,11						
			1,08	680			1,91	
82, 8	54 49	1,04						
			1,01	682			1,67	
82, 3	5 6 11	0,97						
			0,94	683			1,44	
82, 0	17 34	0,91						
			0,88	685			1,27	
81, 3	28 59	0,86						
			0,83	684			1,12	
81, 0	40 23	0,80						
			0,77	684			0,97	
80, 5	51 47	0,75						
82,28	5 0 Mean Solar Time			682,0	680,0	86087,44	1,64	86089,08

A. M. 13th January. Barom. 29,955.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of Clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
74, 2	6 59 32	1,28						
			1,23	685			2,47	
74, 2	7 10 57	1,18						
			1,13	685			2,09	
74, 2	22 22	1,09						
			1,05	686			1,80	
74, 5	33 48	1,01						
			0,98	686			1,57	
74, 8	45 14	0,94						
			0,91	686			1,35	
75, 0	56 40	0,88						
			0,85	686			1,18	
75, 2	8 8 6	0,82						
			0,80	688			1,05	
76, 0	19 24	0,77						
			0,75	688			0,92	
76, 6	31 2	0,71						
74,97	7 40 Mean Solar Time			686,25	684,25	86089,01	1,55	86090,56

P. M. 13th January. Barom. 29,91.

80, 1	5 7 27	1,20						
			1,15	684			2,16	
79, 8	18 51	1,10						
			1,06	685			1,84	
79, 4	30 16	1,02						
			0,99	686			1,60	
79, 0	41 42	0,96						
			0,93	686			1,41	
78, 8	53 8	0,90						
			0,87	686			1,24	
78, 3	6 4 34	0,84						
			0,82	687			1,10	
78, 1	16 1	0,79						
79,07	5 37 Mean Solar Time			685,67	683,67	86088,79	1,56	86090,35

A. M. 14th January, 1823, Galapagos.
Clock losing 59^s,36 at a mean rate. } Barom. 29,94.

Temp. Fahrenheit.	Time of co-incidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of Clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°						
76, 5	7 17 30	1,25						
			° 1,20	690			2,35	
77, 2	29 00	1,16						
			1,13	692			2,09	
77, 6	41 32	1,09						
			1,06	691			1,84	
78, 5	52 3	1,02						
			0,98	690			1,57	
79, 2	8 3 33	0,94						
			0,91	692			1,35	
80, 2	15 5	0,88						
			0,85	693			1,18	
81, 2	26 38	0,81						
			0,79	691			1,02	
83, 0	38 9	0,76						
			0,74	691			0,89	
84, 6	49 40	0,71						
79,77	8 4 Mean Solar Time			691,25	689,25	86090,83	1,54	86092,37

P. M. 14th January. Barom. 29,89.

Temp. Fahrenheit.	Time of co-incidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of Clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
	h. m. s.							
86, 6	4 4 50	1,31						
			1,26	685			2,60	
86, 5	16 15	1,21						
			1,16	686			2,20	
85, 2	27 41	1,12						
			1,08	688			1,91	
84, 7	39 9	1,03						
			0,99	690			1,60	
83, 5	50 39	0,96						
			0,93	690			1,41	
83, 2	5 2 9	0,90						
			0,88	692			1,27	
83, 3	13 41	0,85						
			0,82	691			1,10	
83, 2	25 12	0,79						
			0,76	691			0,94	
83, 0	36 43	0,73						
84,36	4 47 Mean Solar Time			689,125	687,13	86090,06	1,63	86091,69

A. M. 15th January. Barom. 29,94.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of Clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
75, 3	6 36 9	1,28	1,24	690			2,51	
76, 0	47 39	1,19	1,14	689			2,12	
76, 8	59 08	1,10	1,06	692			1,84	
77, 0	7 10 40	1,02	0,98	693			1,57	
77, 1	22 13	0,95	0,92	694			1,38	
77, 3	33 47	0,89	0,85	692			1,18	
78, 2	45 19	0,82	0,79	691			1,02	
80, 0	56 50	0,77	0,74	690			0,89	
81, 0	8 8 20	0,71						
77,63	7 19 Mean Solar Time			691,375	689,38	86090,88	1,56	86092,44

P. M. 15th January. Barom. 29,895.

°	h. m. s.							
86, 8	4 3 15	1,30	1,25	681			2,55	
86, 3	14 36	1,20	1,15	682			2,16	
85, 2	25 58	1,10	1,06	684			1,84	
84, 3	37 22	1,01	0,98	685			1,57	
83, 8	48 47	0,95	0,92	687			1,38	
83, 2	5 00 14	0,89	0,85	687			1,18	
82, 6	11 41	0,82	0,80	689			1,05	
82, 0	23 10	0,77	0,74	689			0,89	
81, 7	34 39	0,72						
83,99	4 46 Mean Solar Time			685,5	683,5	86088,74	1,58	86090,32

TABLE I.

Time by the Clock of Transits of Stars at the Galapagos.

Stars.	January 9.	January 10.	January 13.	January 14.	January 15.
1822.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
12 Eridani	7 57 29,28	——	——	7 32 55,50	——
ζ Eridani	——	7 55 16,10	——	7 35 36,95	——
ε Eridani	8 17 29,57	8 12 33,60	——	7 52 55,00	7 47 58,63
δ Eridani	8 27 37,50	8 22 42,45	——	8 3 3,58	7 58 7,02
α Reticuli	——	——	8 45 12,88	8 40 16,80	——
β Eridani	——	9 46 49,10	9 32 5,92	9 27 9,50	——
Rigel	9 58 35,50	9 53 39,98	9 38 57,37	9 34 1,02	——
γ Orionis	10 8 5,80	10 3 11,25	9 48 28,87	9 43 31,57	——
δ Orionis	10 15 27,85	10 10 33,50	9 55 50,53	9 50 53,75	——
ε Orionis	10 19 43,37	10 14 48,30	10 00 5,37	9 55 8,18	——
ζ Orionis	10 24 17,68	10 19 23,05	10 4 40,40	9 59 43,20	(5th Wire) 9 55 37,50
Do. (5th wire)	25 8,03	20 13,80	5 31,00	0 34,20	——
κ Orionis	10 31 47,70	——	——	10 7 13,08	——

TABLE II.

Transits of the Sun.

Time by Clock at the moment of mean Noon.

January 9.	January 10.	January 11.	January 12.	January 13.	January 14.	January 15.
h. m. s.	m. s.	m. s.	m. s.	m. s.	m. s.	m. s.
12 9 23,82	8 23,27	7 23,27	6 26,90	5 28,67	4 28,65	3 27,27

From these two Tables (which are formed from the last column of the Transit Table, pages 226 and 227,) the following Rates have been computed, by comparing the transits of each night with those of each of the other nights, when the same stars were observed, dividing the difference by the number of days in the interval, and subtracting from the quotient $3^m\ 55^s,91$, the acceleration in one day; to this rate $0^s,16$ have been added for the additional loss of the clock in four minutes, to obtain the rate in a mean solar day.

The sun was fortunately observed every day throughout the whole of these operations; so that by comparing the time by clock at the moment of mean noon of each day, with that on each succeeding day, the rate for 21 separate intervals is obtained.

TABLE III.

Rate of the Clock by the Stars. Losing.

1822. Stars.	Jan. 9, to Jan. 10.	9 to 13	9 to 14	9 to 15	10 to 13	10 to 14	10 to 15	13 to 14	13 to 15	14 to 15
	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
12 Eridani	—	—	59,01	—	—	—	—	—	—	—
ζ Eridani	—	—	—	—	—	59,04	—	—	—	—
ε Eridani	60,22	—	59,16	59,41	—	58,90	59,24	—	—	60,62
δ Eridani	59,30	—	59,03	59,33	—	58,97	59,34	—	—	60,81
α Reticuli	—	—	—	—	—	—	—	60,33	—	—
β Eridani	—	—	—	—	58,64	59,15	—	60,67	—	—
Rigel	59,77	58,78	59,14	—	58,45	58,99	—	60,60	—	—
η Orionis	58,80	58,48	59,10	—	58,37	59,17	—	61,55	—	—
δ Orionis	58,60	58,58	59,07	—	58,57	59,19	—	61,03	—	—
ε Orionis	59,32	58,75	59,29	—	58,56	59,28	—	61,44	—	—
ζ Orionis	58,88	58,57	59,14	—	58,47	59,20	—	61,45	—	—
Do. (5th wire)	—	—	—	59,34	—	—	59,51	—	61,00	60,95
κ Orionis	—	—	59,17	—	—	—	—	—	—	—
Mean by Stars.	59,27	58,63	59,12	59,36	58,51	59,10	59,36	61,01	61 0	60,79
Times to which the above are due (mean solar time.)	10th $9^h\ 30^m$ A. M.	11th $9^h\ 56^m$ P. M.	12th $9^h\ 17^m$ A. M.	12th $8^h\ 42^m$ P. M.	12th $9^h\ 51^m$ A. M.	12th $9^h\ 11^m$ P. M.	13th $8^h\ 41^m$ A. M.	14th $9^h\ 34^m$ A. M.	14th $9^h\ 56^m$ P. M.	15th $8^h\ 33^m$ A. M.

Note. $0^s,16$ have been added to each of the rates by the stars, being the loss in $3^m\ 56^s$ to obtain the rate for a mean solar day.

TABLE IV. *Rate by the Sun. Losing.*

9th to 10th	9th to 11th	9th to 12th	9th to 13th	9th to 14th	9th to 15th	10th to 11th	10th to 12th	10th to 13th	10th to 14th	10th to 15th	11th to 12th	11th to 13th	11th to 14th	11th to 15th	12th to 13th	12th to 14th	12th to 15th	13th to 14th	13th to 15th	14th to 15th
$60^s,55$	$60^s,03$	$58^s,97$	$58^s,79$	$59^s,03$	$59^s,43$	$59^s,51$	$58^s,18$	$58^s,20$	$58^s,65$	$59^s,20$	$56^s,86$	$57^s,55$	$58^s,37$	$59^s,12$	$58^s,23$	$59^s,13$	$59^s,88$	$60^s,02$	$60^s,70$	$61^s,38$
Times to which the above rates are due.	8 min. A. Noon 10th	8 min. A. Mid^t. 10th	8 min. A. Noon 11th	8 min. A. Mid^t. 11th	9 min. A. Noon 12th	8 min. A. Mid^t 10th	8 min. A. Noon 11th	8 min. A. Mid^t 11th	9 min. A. Noon 12th	9 min. A. Mid^t. 12th	9 min. A. Mid^t. 11th	9 min. A. Noon 12th	9 min. A. Mid^t. 12th	9 min. A. Noon 13th	9 min. A. Mid^t. 12th	9 min. A. Noon 13th	9 min. A. Mid^t. 13th	9 min. A. Mid^t. 13th	10 min. A. Noon. 14th	10 min. A. Mid^t. 14th

The rates in the foregoing Tables are due to the middle moment of the intervals between the respective transits from whence they were inferred. The mean rates, in the case of the stars, are due to the middle moment between the mean of the times of the transits on the one night, and the mean of the times of the transits on the other. In the case of the sun, the rates are due to the middle moment between the respective apparent noons. In both cases these middle moments are given (in mean solar time), in order to facilitate an inspection of the three following Tables.

TABLE V.

Vibrations of the Pendulum at the Galapagos,

computed at the mean rate of the clock, viz. 86340,64 Vibrations in a mean Solar Day.

Date.	A. M. or P. M.	Middle time of each set of Coincidences.	Barom.	Therm.	Vibrations in 24 hours.	Correction for Temp.	Vibrations in 24 hours. at 68°
		(Mean Solar Time) h. m.	Inches.	°			
January 10	A. M.	10 4	30,02	85,1	86089,41	7,23	86096,64
	P. M.	2 30	29,90	89,4	86087,04	9,05	86096,09
— 11	A. M.	7 23	29,96	76,4	86091,08	3,55	86094,63
	P. M.	5 30	29,90	82,6	86088,56	6,18	86094,74
— 12	A. M.	7 00	29,93	75,0	86088,64	2,96	86091,60
	P. M.	5 00	29,90	82,3	86089,08	6,06	86095,14
— 13	A. M.	7 40	29,96	75,0	86090,56	2,96	86093,52
	P. M.	5 37	29,91	79,1	86090,35	4,69	86095,04
— 14	A. M.	8 4	29,94	79,8	86092,37	4,99	86097,36
	P. M.	4 47	29,89	84,4	86091,69	6,94	86098,63
— 15	A. M.	7 19	29,94	77,6	86092,44	4,06	86096,50
	P. M.	4 46	29,89	84,0	86090,32	6,77	86097,09
		Mean	29,93	80,9			86095,58

The numbers in the above Table have been deduced from the rate of the clock between the 9th and 15th, viz. 59,36, losing. The corrections for temperature are at the rate of +0ˢ,423 for each degree of Fahrenheit above 68°.

TABLE VI.

By the Stars.

From	To	Mean of the Times in the intervals.		Computed Vibrations in a mean solar day.	Mean of Transit before or after Coincidence.	Final Correction for unequal rate.	Corrected Vibrations in a mean solar day.	No. of Stars observed.	Interval of Transits.	
		Day.	h. m.		before					
10 A. M.	10 P. M.	10	0 17	86096,46	$2^h\ 47^m$	+,04	86096,50	7	1	7
10 A. M.	13 P. M.	11	12 20	86095,40	2 24	,00	86095,40	5	4	20
10 A. M.	14 P. M.	12	0 21	86095,58	3 4	—,10	86095,48	9	5	45
10 A. M.	15 P. M.	12	12 18	86095,58	3 36	—,11	86095,47	3	6	18
11 A. M.	13 P. M.	12	0 22	86094,96	2 31	—,08	86094,88	6	3	18
11 A. M.	14 P. M.	12	12 23	86095,34	3 12	—,10	86095,24	9	4	36
11 A. M.	15 P. M.	13	0 19	86095,42	3 38	—,11	86095,31	3	5	15
14 A. M.	14 P. M.	14	0 25	86096,35	2 51	—,09	86096,26	7	1	7
14 A. M.	15 P. M.	14	12 14	86095,75	2 18	—,07	86095,68	1	2	2
15 A. M.	15 P. M.	15	0 2	86095,35	3 29	+,07	86095,42	3	1	3
		(Mean Solar Time.			Mean by the Stars		86095,56	Sum of Factors		171

TABLE VII.

By the Sun.

From	To	Mean of the times in the intervals.		Computed Vibrations in a mean solar day.	Mean of Transit before or after coincidences.	Final Correction for unequal rate.	Corrected Vibrations in a mean solar day.	No. of Stars observed.	Interval of Transits.	
		day.	h. m.							
10 A. M.	10 P. M.	10	0 17	86095,69	B. 0 9	+,01	86095,70	2	2	4
10 A. M.	11 P. M.	10	12 22	86095,91	B. 0 14	+,01	86095,92	2	3	6
10 A. M.	12 P. M.	11	0 15	86095,38	B. 0 7	+,01	86095,39	2	4	8
10 A. M.	13 P. M.	11	12 20	86095,00	B. 0 12	rate uniform	86095,00	2	5	10
10 A. M	14 P. M.	12	0 21	86095,27	B. 0 12	—,01	86095,26	2	6	12
10 P. M.	11 A. M.	10	10 56	86095,22	A. 1 12	+,06	86095,28	2	1	2
10 P. M.	12 A. M.	10	23 36	86095,44	A. 0 32	+,03	86095,47	2	2	4
10 P. M.	13 A. M.	11	11 50	86095,45	A. 0 18	+,01	86095,46	2	3	6
10 P. M.	14 A. M.	12	0 5	86095,47	A. 0 4	Insensible	86095,47	2	4	8
10 P. M.	15 A. M.	12	12 5	86095,48	A. 0 4	—	86095,48	2	5	10
11 P. M.	12 A. M.	11	12 15	86095,67	B. 0 6	—,01	86095,66	2	1	2
11 P. M.	13 A. M.	12	0 18	86095,57	B. 0 9	—,01	86095,56	2	2	4
11 P. M.	14 A. M.	12	12 28	86095,56	B. 0 19	—,02	86095,54	2	3	6
11 P. M.	15 A. M.	13	0 22	86095,56	B. 0 13	—,01	86095,55	2	4	8
12 P. M.	13 A. M.	12	12 20	86095,46	B. 0 11	—,01	86095,45	2	1	2
12 P. M.	14 A. M.	13	0 35	86095,50	B. 0 26	—,02	86095,48	2	2	4
12 P. M.	15 A. M.	13	12 25	86095,51	B. 0 16	—,01	86095,50	2	3	6
13 P. M.	14 A. M.	13	12 50	86095,54	B. 0 41	—,03	86095,51	2	1	2
13 P. M	15 A. M.	14	0 27	86095,54	B. 0 17	—,01	86095,53	2	2	4
14 P. M.	15 A. M.	14	12 3	86095,54	A. 0 7	+,01	86095,55	2	1	2
					Mean by the Sun		86095,49	Sum of Factors.		110

These two last Tables have been calculated according to the method explained at pages 13 and 14 of Captain Kater's second Paper on the Pendulum (1819). It consists in taking the mean of all the vibrations, and all the corresponding (middle) times of the coincidences, embraced by a certain interval, from Table V.; and then comparing the mean rate (59^s,36) with the rate actually ascertained by transits, which embrace the same, or rather a greater interval, but whose middle time corresponds nearly with the mean time of the said vibrations drawn from Table V. The difference between the mean rate and the observed rate is then applied to the mean of the vibrations; and it is only when the mean time of the transits and that of the coincidences do not agree that the final correction is necessary, and also supposing the rate of the clock not uniform.

Observation for the Latitude of the Station, viz. S. W. Point of the Earl of Abingdon's Island, Galapagos.

January 16, 1822. Barom. 29,97 Thermom. 90°.
Sun on the Meridian by the clock, at 12^{h} 12^{m} 34^{s},7.

Face of Instrument.	Times by Clock.	Time from Noon.	Nat. Vers^{d} Sines.	Altitude and Zenith distance of Sun's Upper and Lower Limbs.	Sun's Semi-diameter.	Altitudes of Sun's centre.
	h. m. s.	m. s.		° ′ ″	′ ″	° ′ ″
East	12 2 48,5	9 46,2	908	68 7 30 L	+ 16 17,2	68 23 47,2
	3 56	8 38,7	712	9 8,5 L	+ ——	25 25,7
	5 5,5	7 29,2	533	10 27,5 L	+ ——	26 44,7
	7 2	5 32,7	293	12 46 L	+ ——	29 3,2
	8 20,5	4 14,2	171	13 55 L	+ ——	30 12,2
	9 38	2 56,7	83	14 36 L	+ ——	30 53,2
	12 27	0 7,7	0	15 22,5 L	+ ——	31 39,7
West	16 2	3 27,3	113	21 46 32,5 L	— ——	29 44,7
	18 55,5	6 20,8	384	48 51,5 L	— ——	27 25,7
	20 10	7 35,3	547	50 26 L	— ——	25 51,2
	21 52	9 17,3	820	52 50 L	— ——	23 27,2
	23 20	10 45,3	1100	22 20 U	+ ——	21 22,8
	24 48,5	12 13,8	1424	25 22,5 U	+ ——	18 20,3
	26 23	13 48,3	1812	28 44 U	+ ——	14 58,8
			807,18			

	° ′ ″		
Latitude	0 32 20	Cos.	9,9999808
Declination	20 56 41	Cos.	9,9703123
Altitude	68 30 59	Sec^{t}.	10,4362401
Log. Sine	1	A. C.	5,3144251
Log of	807″,18	(+ 4)	6,9069704
Log Cor. +	424″,55		2,6279287
Correction +	7′ 4″,55		

	° ′ ″
Altitude of ⊙'s centre .	68 24 12,85
Refraction . . .	— 0 21,12
Parallax . . .	+ 0 3,33
Correction . . .	+ 7 4,55
Change of Declination .	+ 0 0,34
⊙'s true meridian zenith distance .	21 29 0,05
Declination . . .	20 56 41, 0S.
Latitude of the Station	0 32 19,05N.

Thus we have obtained 86095,56 vibrations of the pendulum deduced from the stars, and 86095,49 by the sun, in twenty-four hours, mean solar time. These results, however, as Captain KATER has shown in his paper of 1819, are entitled to credit in the ratio of the sum of the factors arising from multiplying the number of stars observed by the days in the

interval between the observation. For the stars we have 171, and for the sun 110; so that the final number of vibrations may be taken as 86095,54.

The ball of the pendulum was twelve feet above the level of low water, the correction for which, by the duplicate ratio of the distances from the earth's centre, is nearly $0^{\text{v}},05$ in twenty-four hours. As the station was the tabular surface of an old stream of lava, not very compact, I suppose the proper multiplier is $\frac{66}{100}$, which will give $0^{\text{v}},03$ for the correction due to this elevation.

The mean height of the barometer was 29,93, and the mean temperature 80°,9, whence it appears that the specific gravity of the pendulum was to that of air, as 7458 to 1, which gives $5^{\text{v}},77$ as a correction to be added to the number of vibrations to arrive at the number it would have made in vacuo; and adding also 0,03 for the elevation, we have 86101,34 for the number of vibrations made by the pendulum, at the level of the sea in vacuo at 68° of FAHRENHEIT, in a mean solar day, at the Galapagos, in latitude 0° 32′ 19″ north, and longitude $90^{\circ}\frac{1}{2}$ west.

The same pendulum in London made 86235,98 vibrations in the same interval, and reduced to the level of the sea. Whence the length of the seconds pendulum at the Galapagos, deduced from the duplicate ratio of these vibrations, and assuming the length of the seconds pendulum in London 39,13929, appears to be 39,0171692, or 39,01717 inches of Sir G. SHUCKBURGH'S scale.

By comparing the lengths of the seconds pendulum at the principal stations in the British survey as ascertained by Captain KATER'S experiments, the diminution of gravity from the pole to the equator, and the resulting ellipticity, are as follows:

Extreme Stations.	Diminution of Gravity from Pole to Equator.	Ellipticity	Length of Equatorlal Pendulum.
Unst in . . 60° 45′ 28″ and Galapagos in 0° 32′ 19″	,0051945	$\frac{1}{289,35}$	39.01715
Portsoy in . . 57 40 59	,0051833	$\frac{1}{288,41}$	.01715
Leith Fort in 55 58 41	,0051632	$\frac{1}{286,76}$	.01718
Clifton . . . 53 27 43	,0051038	$\frac{1}{281,93}$	.01715
Arbury Hill . 52 12 56	,0051316	$\frac{1}{284,18}$	.01744
London . . 51 31 9	,0051083	$\frac{1}{282,31}$	.01715
Shanklin Farm 50 37 24	,0051038	$\frac{1}{281,92}$	.01715
Mean . .	,0051412	$\frac{1}{284,98}$	39.017196

Experiment No. III. at San Blas, in Mexico.

First Series. By Captain HALL.

Comparison of Chronometer 438 with clock at San Blas. (1st series.)

Date.	Chronometer.			Clock.			Difference.		
1822,	h.	m.	s.	h.	m.	s.	h.	m.	s.
Noon, May 14th,	4	16	45,5	11	25	0	7	8	14,50
P. M.	11	45	50,0	6	54	0			10,00
	0	26	50,5	7	35	0			9,50
	0	47	51,0	7	56	0			9,00
	1	28	51,2	8	37	0			8,80
	1	57	51,5	9	6	0			8,50
	2	23	51,7	9	32	0			8,30
Noon, 15th,	4	9	58,5	11	18	0	7	8	1,50
P. M.	11	27	2,5	6	35	0		7	57,50
	11	42	2,8	6	50	0			57,20
	0	23	3,0	7	31	0			57,00
	0	48	3,4	7	56	0			56,60
	1	27	3,7	8	35	0			56,30
	1	56	3,9	9	4	0			56,10
	2	22	4,1	9	30	0			55,90
Noon, 16th May,	4	8	10, 0	11	16	0	7	7	50,00
P. M.	11	36	15, 0	6	44	0			45,00
	0	22	15, 5	7	30	0			44,50
	0	44	15, 7	7	52	0			44,30
	1	20	16, 0	8	28	0			44,00
	1	55	16, 2	9	3	0			43,80
	2	14	16, 5	9	22	0			43,50
Noon, 17th,	4	6	24, 5	11	14	0	7	7	35,50
P. M.	11	32	31, 8	6	40	0			28,20
	0	13	32, 3	7	21	0			27,70
	0	37	32, 5	7	45	0			27,50
	1	17	33, 0	8	25	0			27,00
	1	47	33, 5	8	55	0			26,50
	2	10	33, 7	9	18	0			26,30
Noon, 18th,	4	8	41, 8	11	16	0	7	7	18,20
P. M.	11	28	47, 4	6	36	0			12,60
	0	9	47, 8	7	17	0			12,20
	0	31	48, 0	7	39	0			12,00
	1	11	48, 5	8	19	0			11,50
	1	42	48, 8	8	50	0			11,20
	2	5	49, 0	9	13	0			11,00
Noon, 19th,	4	6	57, 0	11	14	0	7	7	3,00
P. M.	11	24	1, 0	6	31	0		6	59,00
	0	5	1, 3	7	12	0			58,70
	0	27	1, 5	7	34	0			58,50
	1	9	2, 0	8	16	0			58,00
	1	46	2, 3	8	53	0			57,70
	2	1	2,50	9	8	0			57,50
Noon, 20th,	4	6	9, 7	11	13	0	7	6	50,30

Transits observed at San Blas. (1*st series.*)

Date.	Stars.	1st Wire.	2nd Wire.	Mer. Wire.	4th Wire.	5th Wire.	Mean Chron.	Mean Clock.
1822.		h. m. s.	m. s.	m. s.	m. s.	m. s.		h. m. s.
May 14	⊙'s 1st Limb	4 0 56,25	1 23,25	1 50,75	2 16,25	2 44, 0		
	2d Limb	4 3 10,75	3 38,25	4 5,25	4 31,00	4 58, 0		
	Centre	4 2 3,50	2 30,75	2 58,00	3 23,75	3 51, 0	Mean Centre 4 2 57,48	11 11 12,11
							Clock at mean Noon	11 15 10,17
P. M.	β Ursæ Maj.	11 27 17, 5	28 5, 5	28 52, 5	29 38,25	30 26, 0	11 28 52,04	6 37 2,21
	♆ ———	11 36 13,75	36 50, 5	37 27, 0	38 2, 0	38 38, 0	11 37 26,37	6 45 36,45
	δ Leonis	11 41 31, 5	41 59, 5	42 27, 0	42 53,25	43 20, 5	11 42 26,46	6 50 36,46
	γ Ursæ Maj.	12 20 38, 5	21 22, 5	22 7, 5	22 49, 0	23 33, 5	12 22 6,42	7 30 15,97
	δ ———	12 42 34, 5	43 23, 5	44 12,25	44 58, 0	45 47, 0	12 44 11,25	7 52 20,28
	Cor Caroli	1 24 6, 0	24 39, 5	25 12,25	25 43,75	26 17, 0	1 25 11,79	8 33 20,62
	ζ Ursæ Maj.	1 52 38, 5	53 24, 0	54 10, 0	54 53, 5	55 39, 5	1 54 9,25	9 2 17,78
	g ———	1 53 58, 0	54 44, 5	55 29, 5	56 13, 5	56 59,25	1 55 29,04	9 3 37,54
	η ———	2 16 32, 0	17 12,25	17 52,75	18 30, 5	19 11, 0	2 17 51,87	9 26 00,22
15	⊙'s 1st Limb	4 0 34, 0	1 3, 5	1 30, 0	1 56, 0	2 23, 5		
	2d Limb	4 2 49, 5	3 16, 5	3 43, 5	4 9, 5	4 36, 5		
	Centre	4 1 41,75	2 10, 0	2 36,75	3 2,75	3 30, 0	Mean Centre 4 2 36,33	11 10 37,90
							Clock at mean Noon	11 14 36,00
P. M.	β Ursæ Maj.	11 23 0, 0	23 48, 0	24 35, 5	25 20, 5	26 8, 5	11 24 34,67	6 32 32,20
	♆ ———	11 31 55,25	32 32, 5	33 9, 5	33 44, 0	34 21, 0	11 33 8,62	6 41 6,05
	δ Leonis	11 37 15, 5	37 42,25	38 10, 0	38 36, 0	39 3, 5	11 38 9,54	6 46 6,89
	γ Ursæ Maj.	12 16 21,25	17 5, 5	17 50, 5	18 32, 0	19 16,75	12 17 49,42	7 25 46,48
	δ ———	12 38 17, 5	39 6,25	39 55,25	40 41, 0	41 29, 5	11 39 54,12	7 47 50,80
	Cor Caroli	1 19 49, 0	20 22, 0	20 55, 5	21 27, 0	22 0, 5	1 20 54,92	8 28 51,28
	ζ Ursæ Maj.	1 48 20,75	49 7, 0	49 52, 5	50 36, 0	51 22, 0	1 49 51,79	8 57 47,95
	g ———	1 49 40, 5	50 27, 0	51 12, 5	51 56, 0	52 42, 0	1 51 11,75	8 59 7,90
	η ———	2 12 15,25	12 56, 0	13 36, 0	14 13,25	14 53, 5	2 13 35,00	9 21 31,00
16	⊙'s 1st Limb	4 0 14, 0	0 41,00	1 8,75	1 34,25	2 1,75		
	2d Limb	4 2 29, 0	2 56,25	3 23,50	3 49,00	4 16,50		
	Centre	4 1 21,50	1 48,63	2 16,13	2 41,63	3 9,13	Mean Centre 4 2 15,56	11 10 5,62
							Clock at mean Noon	11 14 3,12
P. M.	β Ursæ Maj.	11 18 43, 5	19 31,75	20 19, 5	21 4, 0	21 52, 0	11 20 18,37	6 28 3,52
	♆ ———	11 27 41, 5	28 16, 5	28 52, 5	29 27, 5	30 4, 0	11 28 52,42	6 36 37,49
	δ Leonis	11 33 0, 0	33 26, 5	33 54, 0	34 20, 5	34 48, 0	11 33 53,83	6 41 38,86
	γ Ursæ Maj.	12 12 3, 5	12 49, 5	13 32, 5	14 14, 5	14 59, 5	12 13 32,00	7 21 16,58
	δ ———	12 33 59,75	34 49, 0	35 37,75	36 21, 0	37 10,25	12 35 35,92	7 43 20,31
	Cor Caroli	1 15 32, 0	16 4,75	16 38, 5	17 9, 5	17 43, 0	1 16 37,71	8 24 21,74
	ζ Ursæ Maj.	1 44 4, 5	44 49, 0	45 35, 0	46 22, 5	47 5, 0	1 45 35,23	8 53 19,13
	g ———	1 45 23,25	46 8, 0	46 55, 5	47 44, 0	48 25,25	1 46 55,25	8 54 39,15
	η ———	2 7 58, 0	8 38, 5	9 18,75	9 56, 5	10 36,75	2 9 17,87	9 17 1,42

Date.	Stars.	1st Wire.	2nd Wire.	Mer. Wire.	4th Wire.	5th Wire.	Mean Chron.	Mean Clock.
1822.		h. m. s.	m. s.	m. s.	m. s.	m. s.	h. m. s.	h. m. s.
May 17	⊙'s 1st Limb	3 59 57, 0	0 3,25	0 50,75	1 17, 0	1 44,25		
	⊙'s 2d Limb	4 2 11, 5	2 38, 5	3 6, 0	3 32, 0	3 58,75		
	⊙'s Centre	4 1 4,25	1 31,00	1 58,37	2 24,50	2 51,50	Mean Centre 4 1 58,00	11 9 33,54
							Clock at mean Noon	11 13 29,84
P. M.	β Ursæ Maj.	11 14 29, 0	15 17, 0	16 4, 5	16 49, 5	17 37, 5	11 16 3,67	6 23 32,03
	ψ ———	11 23 24, 5	24 1,75	24 38, 0	25 13, 0	25 50, 0	11 24 37,54	6 32 5,82
	δ Leonis	11 28 44, 0	29 11,25	29 38,75	30 5, 0	30 32, 5	11 29 38,37	6 37 6,40
	γ Ursæ Maj.	12 7 49,75	8 34, 5	9 19, 0	10 1, 0	10 46, 5	12 9 18,29	7 16 46,02
	δ ———	12 29 46, 5	30 35, 0	31 23,75	32 9, 5	32 58,25	12 31 22,79	7 38 50,36
	Cor Caroli	1 11 17,75	11 51, 0	12 24, 0	12 56, 5	13 29, 0	1 12 23,71	8 19 50,77
	ζ Ursæ Maj.	1 39 50,25	40 36, 0	41 22,25	42 5,75	42 51, 5	1 41 21,33	8 48 47,90
	g ———	1 41 10,25	41 56, 0	42 42, 0	43 25, 0	44 11, 0	1 42 41,04	8 50 7,61
	η ———	2 3 44,00	4 24,25	5 4,25	5 42,25	6 22, 5	2 5 3,58	9 12 29,94
18	⊙'s 1st Limb	3 59 40, 0	0 6,25	0 34, 0	0 59, 5	1 27, 0		
	⊙'s 2d Limb	4 1 53,75	2 20,50	2 48,25	3 14, 0	3 42, 0		
	⊙'s Centre	4 0 46,87	1 13,37	1 41,12	2 6,75	2 34,50	Mean Centre 4 1 40,62	11 8 58,89
							Clock at mean Noon	11 12 53,49
P. M.	β Ursæ Maj.	11 10 15,00	11 2,75	11 50, 5	12 35,25	13 23,25	11 11 49,54	6 19 2,31
	ψ ———	11 19 10,75	19 48, 5	20 24, 5	20 59,25	21 36, 0	11 20 23,91	6 27 36,59
	δ Leonis	11 24 29,75	24 57, 5	25 25, 0	25 50,75	26 18, 0	11 25 24,33	6 32 36,97
	γ Ursæ Maj.	12 3 35,25	4 20,25	5 4, 5	5 46,75	6 31, 5	12 5 3,79	7 12 16,03
	δ ———	12 25 32, 0	26 20, 5	27 9, 5	27 55,25	28 44, 0	12 27 8,46	7 34 20,51
	Cor Caroli	1 7 3,75	7 36,25	8 9, 0	8 40, 5	9 14, 5	1 8 8,83	8 15 20,37
	ζ Ursæ Maj.	1 35 35,50	36 21, 5	37 7,25	37 51, 0	38 36,25	1 37 6,46	8 44 17,72
	g ———	1 36 55,00	37 41, 0	38 27, 5	39 10,75	39 56,75	1 38 26,41	8 45 37,67
	η ———	1 59 29,75	0 9, 5	0 49,75	1 27, 5	2 7,25	2 0 48,92	9 7 59,98
19	⊙'s 1st Limb	3 59 23, 0	59 49, 5	0 16,25	0 42, 0	1 9,50		
	⊙'s 2d Limb	4 1 36, 5	2 3, 5	2 31,25	2 57, 5	3 25,00		
	⊙'s Centre	4 0 29,75	0 56,50	1 23,75	1 49,75	2 17,25	Mean Centre 4 1 23,44	11 8 26,49
							Clock at mean Noon	11 12 18,69
P. M.	β Ursæ Maj.	11 5 57,75	6 45,75	7 33,25	8 17,75	9 6,00	11 7 32,29	6 14 31,43
	ψ ———	11 14 53,75	15 30, 5	16 7, 0	16 41,50	17 18, 5	11 16 6,37	6 23 5,45
	δ Leonis	11 20 13,00	20 40, 0	21 8,25	21 33, 5	22 1,25	11 21 7,57	6 28 6,40
	γ Ursæ Maj.	11 59 18,25	0 2,75	0 47,50	1 29, 0	2 14,25	0 0 46,54	7 7 45,29
	δ ———	12 21 15, 0	22 4, 0	22 52,25	23 38, 0	24 26, 5	0 22 51,33	7 29 49,88
	Cor Caroli	1 2 46, 0	3 19, 0	3 52, 5	4 24, 0	4 57,25	1 3 51,87	8 10 49,93
	η Ursæ Maj.	1 55 11, 5	55 52, 0	56 31, 5	57 10, 0	57 50,50	1 56 31,17	9 3 28,72
20	⊙'s 1st Limb	3 59 2,50	59 29, 0	59 57, 5	0 23, 0	0 50,75		
	⊙'s 2d Limb	4 1 17,75	1 44, 5	2 12, 0	2 37,75	3 5,00		
	⊙'s Centre	4 0 10,12	1 36,75	1 4,75	1 30,37	1 57,07	Mean Centre 4 1 4,10	11 7 54,40
							Clock at mean Noon	11 11 43,80

Observations of Coincidences at San Blas. (1st series.)

May 14, P.M. Clock losing at a mean rate $34^s,33$. Barom. 29,76.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
85,0	3 33 42	1,33						
85,0	3 44 56	1,23	1,28	674			2,68	
85,0	56 11	1,13	1,18	675			2,28	
85,0	4 7 27	1,04	1,08	676			1,91	
84,9	18 45	0,96	1,00	678			1,63	
84,7	30 2	0,89	0,93	677			1,44	
84,5	41 21	0,82	0,86	679			1,18	
84,5	52 40	0,77	0,79	679			1,02	
84,6	5 3 59	0,71	0,74	679			0,89	
84,8				677,12	675,12	86110,57	1,63	86112,20
May 15th. A. M.					Barom. 29,80.			
79,6	5 13 29	1,30						
,7	24 47	1,20	1,25	678			2,55	
,8	36 7	1,11	1,15	680			2,16	
,9	47 25	1,03	1,07	678			1,81	
80,1	58 44	0,96	0,99	679			1,60	
,3	6 10 6	0,89	0,93	682			1,41	
,5	21 27	0,82	0,85	681			1,21	
,7	32 48	0,76	0,79	681			1,02	
,8	44 9	0,71	0,73	681			0,89	
80,2				680,0	678,0	86111,65	1,58	86113,23
P. M. 15th May.					Barom. 29,74.			
85,5	3 30 7	1,31						
,3	41 21	1,21	1,26	674			2,59	
,1	52 36	1,12	1,16	675			2,20	
,1	4 3 53	1,04	1,08	677			1,91	
,2	15 10	0,97	1,01	677			1,63	
,1	26 27	0,90	0,93	677			1,44	
,0	37 45	0,84	0,87	678			1,24	
84,8	49 4	0,79	0,81	679			1,10	
,6	5 0 23	0,74	0,76	679			0,94	
85,1				677,0	675,0	86110,53	1,63	86112,16

May 16th. A. M. Barom. 29,80.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°						
77,5	5 7 47	1,30	°					
,8	19 7	1,19	1,24	680			2,51	
78,1	30 27	1,10	1,14	680			2,16	
,2	41 46	1,02	1,06	679			1,84	
,0	53 7	0,95	0,97	681			1,54	
,2	6 4 28	0,88	0,91	681			1,38	
,3	15 49	0,82	0,85	681			1,18	
,5	27 11	0,77	0,79	682			1,05	
,6	38 33	0,72	0,74	682			0,89	
78,0				680,75	678,75	86111,93	1,57	86113,50
May 16th. P. M.						Barom. 29,75.		
83,5	2 57 27	1,34						
,6	3 8 43	1,23	1,28	676			2,68	
,8	19 58	1,14	1,18	675			2,31	
,9	31 13	1,05	1,09	675			1,94	
,9	42 30	0,98	1,01	677			1,70	
,9	53 48	0,91	0,94	678			1,44	
,9	4 5 6	0,84	0,87	678			1,27	
,9	16 24	0,78	0,81	678			1,07	
,9	27 43	0,73	0,75	679			0,92	
83,8				677	675	86110,53	1,67	86112,20
May 17th. A. M.						Barom. 29,82.		
77,5	5 6 29	1,31						
,5	17 47	1,21	1,26	678			2,60	
,6	29 5	1,12	1,16	678			2,22	
,7	40 23	1,04	1,08	678			1,91	
,8	51 43	0,96	1,00	680			1,63	
,9	6 3 3	0,89	0,92	680			1,38	
78,1	14 22	0,83	0,86	679			1,21	
,5	25 43	0,78	0,80	681			1,07	
,5	37 3	0,72	0,75	680			0,92	
77,9				679,25	677,25	86111,37	1,62	86112,99

May 17, P.M. Clock losing 34^{s},33 at a mean rate. Barom. 29,86.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
84,8	2 57 26	1,34						
,8	3 8 44	1,23	1,28	678			2,68	
,8	20 4	1,13	1,18	680			2,28	
,8	31 23	1,04	1,08	679			1,91	
,7	42 44	0,96	1,00	681			1,63	
,8	54 4	0,89	0,92	680			1,38	
,7	4 5 26	0,83	0,86	682			1,21	
,5	16 48	0,78	0,80	682			1,07	
,3	28 12	0,73	0,75	684			0,92	
84,7				680,75	678,75	86111,93	1,63	86113,56
May 18th. A. M.						Barom. 29,86.		
77,1	5 24 20	1,29						
,1	35 43	1,19	1,24	683			2,51	
,2	47 5	1,10	1,14	682			2,12	
,2	58 28	1,02	1,06	683			1,84	
,6	6 9 53	0,94	0,98	685			1,57	
78,1	21 17	0,88	0,91	684			1,35	
,8	32 43	0,82	0,85	686			1,18	
79,1	44 8	0,76	0,79	685			1,02	
,2	55 33	0,71	0,73	685			0,89	
77,9				684,2	682,2	86113,21	1,56	86114,77
May 18th. P.M.						Barom. 29,83.		
85,1	2 50 25	1,29						
85,0	3 1 39	1,19	1,24	674			2,51	
84,9	12 55	1,10	1,14	676			2,12	
85,0	24 13	1,02	1,06	678			1,84	
85,0	35 32	0,95	0,98	679			1,60	
85,1	46 51	0,88	0,91	679			1,38	
84,9	58 10	0,82	0,85	679			1,18	
84,9	4 9 30	0,77	0,79	680			1,02	
85,0	20 51	0,72	0,74	681			0,92	
85,0				678,25	676,25	86110,99	1,57	86112,56

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
May 19th. A. M.						Barom. 29,83.		
°	h. m. s.	°	°					
78,2	4 58 23	1,32						
,1	5 9 45	1,22	1,25	682			2,55	
,0	21 7	1,13	1,17	682			2,24	
,0	32 31	1,05	1,09	684			1,94	
,1	43 54	0,97	1,01	683			1,66	
,3	55 18	0,89	0,93	684			1,41	
,6	6 6 43	0,83	0,86	685			1,21	
,8	18 9	0,77	0,80	686			1,05	
,9	29 34	0,72	0,74	685			0,89	
78,3				683,88	681,88	86113,09	1,62	86114,71
May 19th. P. M.						Barom. 29,78.		
84,5	3 58 25	1,34						
,2	4 9 43	1,23	1,28	678			2,68	
,2	21 3	1,14	1,18	680			2,31	
,2	32 24	1,05	1,09	681			1,94	
,2	43 46	0,98	1,01	682			1,70	
,0	55 8	0,92	0,95	682			1,47	
83,9	5 6 31	0,86	0,89	683			1,29	
,9	17 54	0,81	0,83	683			1,15	
,8	29 17	0,76	0,78	683			0,99	
84,1				681,5	679,5	86112,21	1,69	86113,90
May 20th. A. M.						Barom. 29,80.		
78,4	5 30 3	1,33						
78,0	41 25	1,23	1,28	682			2,68	
78,3	52 50	1,14	1,18	685			2,28	
78,8	6 4 15	1,05	1,09	685			1,98	
79,0	15 40	0,98	1,01	685			1,70	
79,5	27 4	0,92	0,95	684			1,47	
80,0	38 30	0,86	0,89	686			1,29	
80,1	49 56	0,80	0,83	686			1,12	
80,3	7 1 22	0,75	0,77	686			0,97	
79,1				684,88	682,88	86113,46	1,69	86115,15

TABLE I. (1st. Series.)

Times by the Clock of Transits of Stars at San Blas de California.

Stars.	May 14th.	May 15th.	May 16th.	May 17th.	May 18th.	May 19th.
	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
β Ursæ Maj.	6 37 2,21	6 32 32,20	6 28 3,52	6 23 32,03	6 19 2,31	6 14 31,43
ψ ———	6 45 36,45	6 41 6,05	6 36 37,49	6 32 5,82	6 27 36,59	6 23 5,45
δ Leonis	6 50 36,46	6 46 6,89	6 41 38,86	6 37 6,40	6 32 36,97	6 28 6,40
γ Ursæ Maj.	7 30 15,97	7 25 46,48	7 21 16,58	7 16 46,02	7 12 16,03	7 7 45,29
δ ———	7 52 20,28	7 47 50,80	7 43 20,31	7 38 50,36	7 34 20,51	7 29 49,88
Cor. Caroli	8 33 20,62	8 28 51,28	8 24 21,74	8 19 50,77	8 15 20,37	8 10 49,93
ζ Ursæ Maj.	9 2 17,78	8 57 47,95	8 53 19,13	8 48 47,90	8 44 17,72	
g ———	9 3 37,54	8 59 7,90	8 54 39,15	8 50 7,61	8 45 37,67	
η ———	9 26 00,22	9 21 31,00	9 17 1,42	9 12 29,94	9 7 59,98	9 3 28,72

TABLE II.

Transits of the Sun.

Time by Clock at the moment of mean Noon.

May 14th.	" 15th.	16th.	17th.	18th.	19th.	20th.
h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
11 15 10,17	11 14 36,00	11 14 3,12	11 13 29,84	11 12 53,49	11 12 18,69	11 11 43,80

TABLE III.

Rate of the Clock by the Stars Transits. (Losing.)

Stars.	From 14th to 15th May.	14to16	14to17	14to18	14to19	15to16	15to17	15to18	15to19	16to17	16to18	16to19	17to18	17to19	18to19
	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
Ursæ Maj.	34,10	33,43	34,15	34,06	34,25	32,77	34,17	34,05	34,28	35,58	34,69	34,79	33,81	34,39	34,97
———	34,49	33,57	34,30	34,05	34,29	32,65	34,20	33,91	34,24	35,76	34,54	34,77	33,32	34,27	35,23
Leonis	33,66	32,89	34,11	33,96	34,10	32,12	34,33	34,06	34,21	36,55	35,03	34,91	33,52	34,09	34,66
Ursæ Maj.	33,58	33,78	34,07	34,07	34,23	33,99	34,32	34,24	34,39	34,65	34,36	34,52	34,08	34,45	34,83
———	33,57	34,07	34,06	34,03	34,17	34,58	34,31	34,19	34,32	34,04	33,99	34,23	33,94	34,33	34,72
r. Caroli	33,43	33,53	34,04	34,15	34,23	33,63	34,34	34,39	34,43	35,06	34,77	34,69	34,49	34,51	34,53
Ursæ Maj.	33,92	33,41	34,05	34,10	—	32,91	34,11	34,17	—	35,32	34,79	—	34,27	—	—
———	33,73	33,28	34,07	34,06	—	32,84	34,23	34,17	—	35,63	34,83	—	34,03	—	—
———	33,31	33,49	34,18	34,15	34,39	33,67	34,62	34,43	34,66	35,57	34,81	34,99	34,05	34,70	35,35
ean Proportion for rate n $3^m, 56^s$	33,75 + ,09	33,49 + ,09	34,11 + ,09	34,07 + ,09	34,24 + ,09	33,24 + ,09	34,29 + ,09	34,18 + ,09	34,36 + ,09	35,35 + ,09	34,65 + ,09	34,70 + ,09	33,95 + ,09	34,39 + ,09	34,90 + ,09
ite in a mean olar day.	} 33,84	33,58	34,20	34,16	34,33	33,33	34 38	34,27	34,45	35,44	34,74	34,79	34,04	34,48	34,99
mes to which he above rates re due.	8^h41^m A. M. 15	8^h39^m P.M.15	$8^h 37^m$ A.M.16	$8^h 35^m$ P.M.16	$8^h 24^m$ A.M.17	$8^h 37^m$ A.M.16	$8^h 35^m$ P.M.16	$8^h 33^m$ A.M.17	$8^h 22^m$ P.M.17	$8^h 33^m$ A.M.17	$8^h 31^m$ P.M.17	$8^h 20^m$ A.M.18	$8^h 29^m$ A.M.18	$8^h 18^m$ P.M.18	$8^h 16^m$ A.M.19

TABLE IV.

Rates by Sun's Transit. Clock losing. (1st Series.)

14 to 15	14 to 16	14 to 17	14 to 18	14 to 19	14 to 20	15 to 16	15 to 17	15 to 18	15 to 19	15 to 20	16 to 17	16 to 18	16 to 19	16 to 20	17 to 18	17 to 19	17 to 20	18 to 19	18 to 20	19 to 20
s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	.	s.	s.	s.	s.	s.	s.	s.
34,17	33,52	33,44	34,17	34,30	34,39	32,88	33,08	34,17	34,35	34,44	33,28	34,81	34,81	34,83	36,35	35,57	35,35	34,80	34,84	34,89
$11^h 56^m$ P.M. 14	$11^h 56^m$ A.M. 15	$11^h 56^m$ P.M. 15	$11^h 56^m$ A.M. 16	$11^h 56^m$ P.M. 16	$11^h 56^m$ A.M. 17	$11^h 56^m$ P.M. 15	$11^h 56^m$ A.M. 15	$11^h 56^m$ P.M. 16	$11^h 56^m$ A.M. 17	$11^h 56^m$ P.M. 17	$11^h 56^m$ P.M. 16	$11^h 56^m$ A.M. 17	$11^h 56^m$ P.M. 17	$11^h 56^m$ A.M. 18	$11^h 56^m$ P.M. 17	$11^h 56^m$ A.M. 18	$11^h 56^m$ P.M. 18	$11^h 56^m$ P.M. 18	$11^h 56^m$ A.M. 19	11,56 P.M. 19

TABLE V. (1*st Series*).

Vibrations of the Pendulum at San Blas,

The Clock making 86365,67 Vibrations in a Mean Solar Day.

Date.		Barom.	Thermom.	Difference of Temperature from 68 degrees.	Vibrations in 24 hours.	Corrections for Temperature.	Vibrations in 24 hours, in Temperature 68 degrees.
		Inches.	°	°			
May 14	P. M.	29,76	84,8	16,8	86112,20	+ 7,11	86119,31
15	A. M.	29,80	80,2	12,2	86113,23	+ 5,16	86118,39
	P. M.	29,74	85,1	17,1	86112,16	+ 7,23	86119,39
16	A. M.	29,80	78,0	10,0	86113,50	+ 4,23	86117,73
	P. M.	29,75	83,8	15,8	86112,20	+ 6,68	86118,88
17	A. M.	29,82	77,9	9,9	86112,99	+ 4,19	86117,18
	P. M.	29,86	84,7	16,7	86113,56	+ 7,06	86120,62
18	A. M.	29,86	77,9	9,9	86114,77	+ 4,19	86118,96
	P M.	29,83	85,0	17,0	86112,56	+ 7,19	86119,75
19	A. M.	29,83	78,3	10,3	86114,71	+ 4,36	86119,07
	P. M.	29,78	84,1	16,1	86113,90	+ 6,81	86120,71
20	A. M.	29,80	79,1	11,1	86115,15	+ 4,69	86119,84
Mean		29,80	81,6				86119,15

TABLE VI. 1*st Series.*

By the Stars.				
	Correct Vibrations in a Mean Solar Day.	Number of Stars observed.	Interval of Transits.	Sum of the Factors.
From 15 A. M. to 15 P. M.	86119,38	9	1	9
16	86119,35	9	2	18
17	86118,83	9	3	27
18	86119,03	9	4	36
19	86119,07	7	5	35
16 A. M. to 16 P. M.	86119,30	9	1	9
17	86118,55	9	2	18
18	86118,91	9	3	27
19	86118,99	7	4	28
17 A. M. to 17 P. M.	86117,79	9	1	9
18	86118,72	9	2	18
19	86118,92	7	3	21
18 A. M. to 18 P. M.	86119,64	9	1	9
19	86119,47	7	2	14
19 A. M. to 19 P. M.	86119,23	7	1	7
Mean by Stars -	86119,01	Sum of Factors		285

TABLE VII.

By the Sun.				
	Correct Vibrations in a Mean Solar Day.	Number of Stars observed.	Interval of Transits.	Sum of the Factors.
From 14 P. M. to 15 A. M.	86119,01	2	1	2
16	86119,51	2	2	4
17	86119,37	2	3	6
18	86118,97	2	4	8
19	86118,96	2	5	10
20	86119,09	2	6	12
15 P. M. to 16 A. M.	86120,01	2	1	2
17	86119,54	2	2	4
18	86118,95	2	3	6
19	86118,95	2	4	8
20	86119,10	2	5	10
16 P. M. to 17 A. M.	86119,08	2	1	2
18	86118,43	2	2	4
19	86118,60	2	3	6
20	86118,88	2	4	8
17 P. M. to 18 A. M.	86117,77	2	1	2
19	86118,36	2	2	4
20	86118,80	2	3	6
18 P. M. to 19 A. M.	86118,94	2	1	2
20	86119,33	2	2	4
19 P. M. to 20 A. M.	86119,71	2	1	2
Mean by Sun -	86119,02	Sum of Factors		112

Observations for the Latitude.

San Blas, 20th May, 1822. Barometer 29,78. Thermometer 83°. Chronometer too fast for mean time $4^h\ 4^m\ 45^s$. Polaris on the meridian below the Pole by the Chronometer at $1^h\ 8^m\ 41^s$. True apparent N. P. D. 1° 38′ 28″,46.

Face of Instrument.	Chronometer.	Time from the Meridian.	Nat. Versed Sines.	Observed Zenith Distance and Altitudes.	Altitude.
	h. m. s.	m. s.		° ′ ″	° ′ ″
East	1 6 5	2 36	64	70 3 34,5	19 56 25,5
	1 7 51	0 50	7	5 34	56 26
	1 8 41	0 0	0	3 35	56 25
West	1 14 3	5 22	274	19 56 19	56 19
	1 16 11	7 30	535	56 18	56 18
	1 18 35	9 54	933	56 20,5	56 20,5
			302,17		19 56 22,33 Obs. altitude,

	° ′ ″		
Latitude -	21 32 22	Cosine	9,9685600
Declination	88 21 31	Cosine	8,4570295
Altitude -	19 53 53	Secant	10,0267337
Constant Log. when Stars are obs.			5,3168000
		Constant Log.	3,7691232
Log of	302,17 (+4) -		6,4802470
Correction — 1″,77		Log.	0,2493702

° ′ ″	
19 56 22,33	Obs. altitude,
2 27,48	Refraction.
1,77	Correction.
0,01	2′*r* C
19 53 53,07	True altitude.
+1 38 28,46	Apparent P.D.
21 32 21,53	Latitude.

June 4th, 1822.
Face of Circle, West.

Barometer 29,75. Thermometer 86°.
Sun's declination 22° 26′ 42″,4.

			° ′ ″
Readings	1st Vernier .		80 50 0
	2nd Vernier .		50 10
Obs. merid. alt. ⊙'s L. L.			88 50 5
⊙'s semi-diameter .		+	15 47,14
Refraction . .		—	0,10
True merid. alt. ⊙'s centre			89 5 52,04
⊙'s merid zenith dist.			0 54 7,96
Declination . .			22 26 42,40
Latitude, face west . .			21 32 34,44

June 6th, 1822.
Face of Circle, East.

Barometer 29,80. Thermometer 85°.
Sun's declination 22° 39′ 57″,9.

			° ′ ″
Readings	1st Vernier .		1 23 30
	2nd Vernier .		23 25
Obs. zenith dist. ⊙'s L. L.			1 23 27,5
⊙'s semi-diameter .			15 47,0
Refraction . .		+	1,0
Parallax . .		—	0,78
⊙'s true mer. zenith dist.			1 7 40,72
Declination . . .			22 39 57,9
Latitude, Face East .			21 32 17,18

Latitude, Face West . .	21 32 34,44 N.
East . .	21 32 17,18
Latitude by the Sun . .	21 32 25,81
Polaris . .	21 32 21,53
Latitude of Observatory .	21 32 23,67 N.

Thus we have obtained 86119,01 vibrations by the rates deduced from the transits of stars, and 86119,02 by the sun's transits. But the sums of the factors being respectively 285 and 112, the former determination, or 86119,01, may be taken for the final mean number of vibrations in 24 hours.

The height of the ball of the pendulum was found by levelling, agreeing with a trigonometrical measurement, to be 115 feet; the correction due to which is $\overset{v}{\underset{v}{0{,}47}} \times \frac{6}{10} = \underset{v}{0{,}28}$; this together with that for the buoyancy of the atmosphere, viz. 5,74, gives 6,02 to be added to the mean number of vibrations; and we have 86125,03 for the final number of vibrations which would be made by this pendulum in vacuo at the temperature of 68°, at the level of the sea, in a mean solar day at San Blas, in latitude 21° 32′ 24″ N. and longitude 105° 15′ W.

From the above data, and the number of vibrations made by the same pendulum in London, after returning to England,* viz. 86236,95, together with the known length of the second's pendulum in London, the length of the second's pendulum at San Blas is found to be 39,03776 inches; and comparing this with the lengths of the second's pendulum determined by CAPTAIN KATER at the principal British stations, we obtain the following expressions for the diminution of gravity from the pole to the equator, and ellipticity of the earth, together with the lengths of the equatorial pendulum by each comparison.

Stations compared with San Blas, in Lat. 21° 32′ 24″ N.		Diminution of Gravity from Pole to Equator.	Ellipticity.	Length of Equat. Pendul.
Unst	in Lat. 60 45 28 N.	,0054703	$\frac{1}{314{,}44}$	39,00899
Portsoy - - -	57 40 59	,0054789	$\frac{1}{315{,}30}$	,00895
Leith - - - -	55 58 41	,0054683	$\frac{1}{314{,}25}$	,00901
Clifton - - -	52 27 43	,0054328	$\frac{1}{310{,}78}$	,00920
Arbury Hill - -	52 12 55	,0054819	$\frac{1}{315{,}60}$	,00893
London - - -	51 31 8	,0054452	$\frac{1}{311{,}98}$	,00912
Shanklin Farm -	50 37 24	,0054505	$\frac{1}{312{,}50}$	,00910
	Mean	,0054611	$\frac{1}{313{,}55}$	39,00904

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* See the Remarks after the Experiment given in the Appendix.

Experiment No. IV. 2nd Series at San Blas;

By Mr. H. Foster.

Comparisons of Clock with Chronometer 438, *at San Blas;*

(2nd Series.)

Date.	Barometer.	Therm. Fahrenheit.	Time by Chronometer.	Time by Clock.	Clock fast of Chronometer.
1822,	Inches.	°	h. m. s.	h. m. s.	h. m. s.
May 26th, Noon,	29,88	86,2	4 6 51,5	12 9 0	8 2 8,5
P.M.	——	——	11 35 53,9	7 38 0	2 6,1
	——	——	1 6 54,7	9 9 0	2 5,3
	29,87	84,5	2 13 55,0	10 16 0	2 5
27th, Noon,	29,86	86,5	4 4 1,0	12 6 0	8 1 59,0
P.M.	——	——	11 31 5,1	7 33 0	1 54,9
	——	——	11 54 5,2	7 56 0	1 54,8
	——	——	12 35 5,7	8 37 0	1 54,3
	——	——	1 5 5,9	9 7 0	1 54,1
	29,82	84,8	2 12 6,5	10 14 0	1 53,5
28th, Noon,	29,79	86,2	4 4 12,0	12 6 0	8 1 48,0
P. M.	——	——	11 26 16,2	7 28 0	1 43,8
	——	——	11 48 16,5	7 50 0	1 43,5
	29,76	85,2	1 23 17,4	9 25 0	1 42,6
29th Noon,	29,77	87,0	4 3 25,9	12 5 0	8 1 34,1
P. M.	——	——	11 43 30,7	7 45 0	1 29,3
	——	——	12 24 31,0	8 26 0	1 29,0
	——	——	12 56 31,3	8 58 0	1 28,7
	——	——	1 19 32,5	9 21 0	1 28,5
	——	——	1 45 31,8	9 57 0	1 28,2
	29,78	85,0	2 3 32,0	10 5 0	1 28,0
30th, Noon.	29,78	88,0	4 4 38,9	12 6 0	8 1 21,1

Transits of Stars. (2nd Series.)

Date.	Stars.	1st. Wire	2nd. Wire.	Mer. Wire.	4th Wire.	5th Wire.	Mean Chron.	Mean Clock.
1822.		h. m. s.	m. s.	m. s.	m. s.	m. s.	h. m. s.	h. m. s.
May 26.	⊙'s 1st Limb	3 57 24, 0	57 52, 0	58 19, 0	58 45, 5	59 13, 5		
	2d Limb	3 59 29, 0	59 57, 0	0 24, 5	0 50, 5	1 18, 0		
	Centre	3 58 26,50	58 54,50	59 21,75	59 48, 0	0 15,75	3 59 21,37	12 1 29,91
							Clock at mean Noon	12 4 50,97
P. M.	γ Ursæ Maj	——	——	11 30 44, 0	31 26, 0	——	11 31 5, 0	7 33 11,13
	g ———	1 2 34, 7	3 21, 0	4 6, 5	4 50, 0	5 35, 5	1 4 5,70	9 6 11,02
	λ Bootis	1 54 14, 0	54 51, 7	55 29, 0	56 4, 5	56 42, 0	1 55 28,37	9 57 33,47
27	⊙'s 1st Limb	3 57 4, 5	57 32, 0	58 00, 0	56 26, 2	58 54, 0		
	2d Limb	3 59 16, 5	59 44, 0	0 11, 5	0 38, 0	1 5, 5		
	Centre	3 58 10,50	58 38, 0	59 5,75	59 32,10	59 59,75	3 59 5,31	12 1 4,34
							Clock at mean Noon	12 4 18,34
P. M.	γ Ursæ Maj.	11 24 56, 5	25 41, 0	26 25, 0	27 7, 2	27 51, 5	11 26 24,37	7 28 19,32
	δ ———	11 46 53, 5	47 42, 0	48 30, 5	59 16, 5	50 4, 0	11 48 29,50	7 50 24,36
	Cor Caroli	——	12 28 57, 0	29 30, 2	30 1, 5	30 34, 5	12 29 29,72	8 31 24,08
	g Ursæ Maj.	12 58 16, 5	59 2, 0	59 47, 5	00 30, 0	1 15, 5	12 59 46,50	9 1 40,66
	γ Bootis	1 49 55, 0	50 33, 0	51 10, 0	51 45, 5	52 24, 0	1 51 9,58	9 53 3,28
	θ ———	1 59 16, 0	59 58, 0	00 40, 5	1 20, 5	2 2, 0	2 0 39,58	10 2 33,20
	γ ———	2 5 19, 5	5 52, 5	6 25, 5	6 56, 5	7 30, 0	2 6 24,92	10 8 18, 48
28	⊙'s 1st Limb	3 56 53, 5	57 21, 0	57 49, 0	58 15, 0	58 43, 0		
	2d Limb	3 58 54, 0	59 21, 0	59 48, 5	00 15, 0	00 43, 0		
	Centre	3 57 53,75	58 21, 0	58 48,75	59 15, 0	59 43, 0	3 58 48,37	12 00 36,42
							Clock at Mean Noon	12 3 44,12
P. M.	γ Ursæ Maj.	11 20 36, 5	21 20, 5	22 5, 0	22 47, 2	23 31, 2	11 22 4, 23	7 23 48,07
	δ ———	11 42 34, 0	43 22, 5	44 10, 5	44 56, 5	45 45, 0	11 44 9, 84	7 45 53,38
	η ———	1 16 30, 0	17 10, 5	17 51, 0	18 28, 5	19 8, 5	1 17 49, 92	9 19 32,57
29	⊙'s 1st Limb	3 56 41, 5	57 8, 5	57 36, 2	58 2, 0	58 30, 0		
	2d Limb	3 58 38, 0	59 5, 0	59 33, 0	59 59, 0	00 27, 0		
	Centre	3 57 39,75	58 6,75	58 34, 6	59 0, 5	59 28, 5	3 58 34,11	12 0 8,26
							Clock at Mean Noon	12 3 8,36
P. M.	δ Ursæ Maj.	11 38 15, 8	39 4, 5	39 53, 5	40 38, 5	41 27, 0	11 39 52,13	7 41 21,47
	Cor Caroli	12 19 46, 0	20 19, 2	20 53, 0	21 24, 0	21 57, 0	12 20 52,03	8 22 21,07
	g Ursæ Maj.	12 49 39, 0	50 24, 0	51 10, 5	51 53, 5	52 39, 5	12 51 9,50	8 52 38,26
	η ———	1 12 14, 0	12 53, 0	13 33, 5	14 10, 5	14 51, 0	1 13 32,58	9 15 1,15
	λ Bootis.	1 41 18, 5	41 56, 0	42 33, 5	43 9, 0	43 46, 0	1 42 32,75	9 44 0,98
	θ ———	1 50 37, 2	51 22, 0	52 3, 0	52 43, 0	53 24, 5	1 52 2,12	9 53 30,24
	γ ———	1 56 42, 5	57 15, 0	57 48, 2	58 19, 5	58 52, 8	1 57 47,70	9 59 15,76
30	⊙'s 1st Limb	3 56 16, 0	56 44, 0	57 11, 5	58 6, 0			
	2d Limb	3 58 32, 0	58 59, 0	59 27, 0	00 22, 0			
	Centre	3 57 24, 0	57 51,50	58 19,25	58 45, 5	59 14, 0	3 58 18,92	11 59 40,08
							Clock at Mean Noon	12 2 32,71

Observations of Coincidences at San Blas. (2nd Series.)

May 27, A.M. Clock losing $35^s,06$ at a mean rate. Barom. 29,87.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°						
81,5	6 12 53	1,32	°					
,7	24 12	1,22	1,27	679			2,64	
,9	35 32	1,13	1,17	680			2,24	
82,0	46 52	1,05	1,09	680			1,94	
,3	58 13	0,97	1,01	681			1,67	
83,0	7 9 36	0,90	0,93	683			1,42	
,5	20 58	0,83	0,86	682			1,21	
,8	32 20	0,77	0,80	682			1,05	
84,0	43 44	0,72	0,74	684			0,90	
82,6				681,37	679,37	86111,43	1,63	86113,06
May 27th. P.M.						Barom. 29,80.		
87,1	3 50 57	1,30						
,1	4 2 15	1,20	1,25	678			2,55	
,1	13 14	1,11	1,15	679			2,17	
,2	24 54	1,02	1,06	680			1,84	
,2	36 14	0,95	0,98	680			1,57	
,2	47 34	0,88	0,91	680			1,36	
,2	58 57	0,82	0,85	683			1,18	
,2	5 10 17	0,77	0,79	680			1,03	
,0	21 39	0,72	0,74	682			0,90	
87,1				680,25	678,25	86111,02	1,57	86112,59
May 28th. A. M.						Barom. 29,80.		
82,6	6 00 5	1,33						
,8	11 25	1,22	1,27	680			2,64	
,9	22 46	1,12	1,17	681			2,24	
,9	34 8	1,05	1,08	682			1,91	
83,0	45 30	0,96	1,00	682			1,64	
,2	56 52	0,90	0,93	682			1,41	
,6	7 8 16	0,83	0,86	684			1,21	
,8	19 39	0,77	0,80	683			1,05	
84,0	32 2	0,71	0,74	683			0,90	
83,1				682,12	680,12	86111,71	1,62	86113,33

May 28, P. M. Clock losing 35^s,06 at a mean rate. Barom. 29,76.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°						
87,9	3 53 32	1,34	°					
,9	4 4 52	1,24	1,29	680			2,72	
,9	16 13	1,14	1,19	681			2,31	
,9	27 34	1,05	1,09	681			1,95	
,8	38 57	0,99	1,02	683			1,70	
,8	50 19	0,92	0,95	682			1,48	
,8	5 1 42	0,85	0,88	683			1,27	
,8	13 6	0,78	0,81	684			1,08	
,7	24 31	0,72	0,75	685			0,92	
87,8				682,37	680,37	86111,80	1,68	86113,48
May 29th. A. M.						**Barom. 29,77.**		
81,8	6 1 51	1,32						
,5	13 10	1,22	1,27	679			2,64	
,5	24 30	1,14	1,18	680			2,28	
,8	35 51	1,08	1,11	681			2,01	
82,0	47 12	1,00	1,04	681			1,77	
,2	58 34	0,92	0,96	682			1,51	
,4	7 9 57	0,87	0,89	683			1,30	
,9	21 18	0,79	0,83	681			1,12	
83,0	32 40	0,73	0,76	682			0,94	
				681,12	679,12	86111,34	1,69	86113,03
May 29th. P. M.						**Barom. 29,75.**		
88,5	4 1 20	1,32						
,2	12 38	1,22	1,27	678			2,64	
,1	23 57	1,13	1,18	679			2,28	
,1	35 17	1,05	1,09	680			1,94	
,1	46 37	0,97	1,01	680			1,67	
1	58 57	0,90	0,93	680			1,42	
,1	5 9 19	0,84	0,87	682			1,24	
,0	20 41	0,78	0,81	682			1,07	
,0	32 5	0,72	0,75	684			0,92	
88,1				680,62	678,62	86111,16	1,65	86112,81

May 30th. A. M.					Barom. 29,78.			
Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°						
79,8	5 58 9	1,34	°					
,8	6 9 31	1,24	1,29	682			2,72	
,8	20 52	1,15	1,19	681			2,32	
,8	32 13	1,08	1,11	681			2,02	
,8	42 34	1,00	1,04	681			1,77	
80,0	54 57	0,92	0,96	683			1,51	
,2	7 6 21	0,85	0,88	684			1,27	
81,0	17 45	0,80	0,82	684			1,10	
,0	29 7	0,75	0,77	682			0,97	
80,1				682,25	680,25	86111,76	1,71	86113,47

TABLE I. *(2d Series.)*

Times by clock at Transits of Stars.

Stars.	May 16th.	27th	28th.	29th.
	h. m. s.	h. m. s.	h. m. s.	h. m. s.
γ Ursæ Majoris	——	7 28 19,32	7 23 48,07	——
Do. 3rd & 4th wires	7 33 11,13	7 28 41,05	7 24 9,94	——
δ Ursæ Maj. . .	——	7 50 24,36	7 45 53,38	7 41 21,47
Cor Caroli . .	——	8 31 24,08	——	8 22 21,07
g Ursæ Maj. . .	9 6 11,02	9 1 40,66	——	8 52 38,26
η Ursæ Maj. . .	——	——	9 19 32,57	9 15 1,15
λ Bootis . . .	9 57 33,47	9 53 3,28	——	9 44 0,98
θ Bootis . . .	——	10 2 33,20	——	9 53 30,24
γ Bootis . . .	——	10 8 18,48	——	9 59 15,76

TABLE II.

Transits of Sun.

Time by clock at Mean Noon, May 26.	27th.	28th.	29th.	30th.
h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
12 4 50,97	12 4 18,34	12 3 44,12	12 3 8,66	12 2 32,71

TABLE III. *(2nd Series.)*

Rates of the Clock by the Stars' Transits. (Losing.)

Stars.	From 26 to 27	26 to 28	26 to 29	27 to 28	27 to 29	28 to 29
	s.	s.	s.	s.	s.	s.
γ Ursæ Maj. -	34,17	34,68	—	35,34	—	—
δ ——	—	—	—	35,07	35,53	36,00
Cor Caroli -	—	—	—	—	35,59	—
g Ursæ Maj. -	34,45	—	35,01	—	35,29	—
η ——	—	—	—	—	—	35,51
λ Bootis -	34,28	—	34,92	—	35,24	—
θ ——	—	—	—	—	35,57	—
γ ——	—	—	—	—	35,45	—
Mean by stars -	34,30	34,68	34,96	35,20	35,44	35,75
Proportional rate for the acceleration $3^m 56^s$.	+,10	+,10	+,10	+,10	+,10	+,10
Rate in a mean solar day	34,40	34,78	35,06	35,30	35,54	35,85
Mean solar Time to which the above rates are due	$8^h 45^m$ A. M. 27	$7^h 24^m$ P. M. 27	$9^h 21^m$ A. M. 28	$7^h 33^m$ A. M. 28	$9^h 6^m$ P. M. 28	$8^h 27^m$ A. M. 29

TABLE IV.

Rates by the Sun's Transits. (Clock losing.)

26 to 27	26 to 28	26 to 29	26 to 30	27 to 28	27 to 29	27 to 30	28 to 29	28 to 30	29 to 30
s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
32,63	33,42	34,10	34,56	34,22	34,84	35,21	35,46	35,70	35,95
$11^h 55^m$ P. M. 26	$11^h 56^m$ A. M. 27	$11^h 56^m$ P. M. 27	$11^h 56^m$ A. M. 28	$11^h 56^m$ P. M. 27	$11^h 56^m$ A. M. 28	$11^h 56^m$ P. M. 28	$11^h 56^m$ P. M. 28	$11^h 57^m$ A. M. 29	$11^h 57^m$ P. M. 29

TABLE V. *(2d. Series.)*

Vibrations of the Pendulum at San Blas,

The Clock making 86364,94 vibrations in a mean solar day.

Date.	Barom.	Therm.	Difference of Temperature from 68°	Vibrations in 24 hours.	Corrections for Temperature.	Vibrations. in 24 hours at Temperature 68 degrees.
	Inches.	°	°		s.	
May 27, A.M.	29,87	82,6	14,6	86113,06	+ 6,18	86119,24
P.M.	29,80	87,1	19,1	86112,59	+ 8,08	86120,67
28, A.M.	29,80	83,1	15,1	86113,33	+ 6,39	86119,72
P. M.	29,76	87,8	19,8	86113,48	+ 8,37	86121,85
29, A. M.	29,77	82,1	14,1	86113,03	+ 5,96	86118,99
P. M	29,75	88,1	20,1	86112,81	+ 8,50	86121,31
30, A.M.	29,78	80,1	12,1	86113,47	+ 5,12	86118,59
Mean	29,79	84,4				86120,05

TABLE VI.

By the Stars.

	Correct Vibrations in a mean solar day.	Number of Stars observed.	Interval of Transits.	Factors.
From 27 May, A. M. to 27, P. M.	86120,61	3	1	3
28	86120,65	1	2	2
29	86120,30	2	3	6
28 A. M. to 28, P. M.	86120,54	2	1	2
29, P. M.	86119,99	6	2	12
29 A. M. to 29, P. M.	86119,36	2	1	2
Mean	86120,24	Sum of Factors		27

TABLE VII.

By the Sun.

	Correct Vibrations in a mean solar day.	Number of Stars observed.	Interval of Transits.	Factors.
From 27 May, P. M. to 28, A. M.	86121,03	2	1	2
29	86120,53	2	2	4
30	86120,04	2	3	6
28 P. M. to 29, A.M.	86120,02	2	1	2
30	86119,54	2	2	4
29 P. M. to 30, A.M.	86119,06	2	1	2
Mean by the Sun	86120,04	Sum of Factors		20

By the transits of the stars we have obtained 86120,24 vibrations made by the pendulum, and by the sun 86120,04; but the sums of the factors for the stars being 27, and those for the sun 20, we have 0,15 to add to the vibrations given by the sun to arrive at 86120,19, the mean number of vibrations in 24 hours.

The ball of the pendulum was elevated above the level of the sea 115 feet, the correction due to which is $0,47 \times \frac{6}{10} = 0,28$; this, together with 5,71, the correction for buoyancy of the atmosphere, gives 5,99 as the final sum to be added to the mean number of vibrations in 24 hours, which gives 86126,18 for the number of vibrations made by the pendulum in vacuo at the level of the sea, and temperature 68° at San Blas de California, in latitude 21° 32′ 24″ N, longitude 105° 15′ West.

From the above data, and the length of the second's pendulum in London, determined after the return,* the length of the second's pendulum at San Blas appears to be 39,03881 inches, and comparing this with the lengths ascertained at different places, by Captain KATER, we obtain the following results:—

Stations compared with San Blas, in Lat. 21° 32′ 24″ N.	Diminution of Gravity from Pole to Equator.	Ellipticity.	Length of the Equat. Pend.
Unst - in Lat. 60 45 28 N.	,0054273	$\frac{1}{310,25}$	39,01026
Portsoy - - - 57 40 59	,0054323	$\frac{1}{310,73}$	,01024
Leith Fort - - 55 58 41	,0054193	$\frac{1}{309,48}$	,01031
Clifton - - - 52 27 43	,0053799	$\frac{1}{305,75}$	,01052
Arbury Hill - - 52 12 55	,0054268	$\frac{1}{310,21}$	,01027
London - - - - 51 31 8	,0053888	$\frac{1}{306,59}$	,01047
Shaklin Farm - 50 37 24	,0053923	$\frac{1}{306,92}$	,01045
Mean	,0054095	$\frac{1}{308,56}$	39,01036

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* See Remarks after the Experiment given in the Appendix.

Experiment No. V. at Rio de Janeiro.

1st *Series. By Captain* HALL.

Comparisons of Clock with Chronometer 438 *at Rio de Janeiro.*

Date.	Barom.	Therm.	Chronometer.	Clock.	Difference.
1822.	Inches.	°	h. m. s.	h. m. s.	Clock Fast. m. s.
Sept. 28 Noon.	29,86	72	11 19 50,00	11 49 0,0	29 10,00
29	29,92	73	11 13 10,00	11 42 0,0	28 50,00
P. M.			5 8 15,00	5 37 0,0	28 45, 0
			6 35 16, 0	7 4 0,0	28 44, 0
30	29,88	74	11 24 29, 5	11 53 0,0	28 30, 5
P. M.			4 55 34, 5	5 24 0,0	28 25, 5
			6 7 35, 5	6 36 0,0	28 24, 5
Oct. 1.	29,81	74½	11 30 51, 5	12 9 0,0	28 8, 5
P. M.			6 39 58, 5	7 8 0,0	28 1, 5
			7 41 59, 5	8 10 0,0	28 0, 5
2	29,82	75	11 16 14, 5	11 44 0,0	27 45, 0
4	30,00	71,9	11 8 53, 5	11 36 0,0	27 6, 5
5	29,90	71,90	11 9 12, 5	11 36 0,0	26 47, 5
P. M.			5 20 17, 5	5 47 0,0	26 42, 5
			5 47 18, 0	6 14 0,0	26 42, 0
			7 36 19, 5	8 3 0,0	26 40, 5

Transits of Stars at Rio de Janeiro. (1st Series.)

Date.	Stars.	1st Wire.	2nd Wire.	Mer. Wire.	4th Wire.	5th Wire.	Mean Chron.	Mean Clock.	
		h. m. s.	m. s.	m. s.	h. m. s.	m. s.	h. m. s.	h. m. s.	
1822. Sept. 28	⊙'s 1st Limb	11 4 52,75	5 19,00	5 43,00	6 8,50	6 34,00			
	⊙'s 2d Limb	11 7 1,00	7 27,25	7 52,00	8 17,50	8 43,00			
	⊙'s Centre	11 5 56,87	6 23,12	6 47,50	7 13,00	7 38,50	11 6 47,75	11 35 57,92	
							Clock at mean Noon	11 45 13,72	
29	⊙'s 1st Limb	11 4 12,00	4 38,00	5 2,75	5 28,00	5 53,50			
	⊙'s 2d Limb	11 6 21,25	6 46,75	7 11,50	7 36,75	8 3,00			
	⊙'s Centre	11 5 16,62	5 42,37	6 7,12	6 32,37	6 58,25	11 6 7,31	11 34 57,40	
							Clock at mean Noon	11 44 33,15	
P. M.	α Lyræ -	——	——	——	5 14 55, 0	15 29, 0	5 15 12,00	5 43 56,91	Mean of 4th and 5th wires.
	β ——	5 25 57,00	26 27,75	26 57,25	27 27,50	27 58,62	5 26 57,56	5 55 42,34	
	γ ——	5 34 42,50	35 13,25	35 42,50	36 12,50	36 43,25	5 35 42,74	6 4 27,44	
	κ Cygni -	5 54 56,25	55 39,00	56 20,25	57 2,00	——	5 55 59,37	6 24 43,90	Mean of four first wires κ Cygni.
	κ Cygni -	5 54 56,25	——	56 20,25	——	——	5 55 38,25	6 24 22,78	Mean of 1st and Mer. wire κ Cygni.
	γ Aquilæ -	6 20 14,25	20 40,75	21 5,25	21 31,50	21 57,50	6 21 5,75	6 49 49,95	
	α Aquilæ -	6 24 32,00	24 58,00	25 22,75	25 48,75	26 14,50	6 25 23,12	6 54 7,28	
30	⊙'s 1st Limb	11 3 31,00	3 57, 0	4 21,00	4 47,00	5 12,50			
	⊙'s 2d Limb	11 5 39,75	6 5,75	6 30,50	6 56,12	7 21,62			
	⊙'s Centre	11 4 35,37	5 1,37	5 25,75	5 51,56	6 17,06	11 5 26,14	11 33 56,89	
							Clock at mean Noon	11 43 52,21	
P. M.	α Lyræ -	5 9 1,25	9 34,62	10 6,12	10 39,00	11 12,00	5 10 6,52	5 38 31,81	
	Ditto	——	——	——	10 39,00	11 12,00	5 10 55,50	5 39 20,79	Mean of 4th and 5th wires α Lyræ.
	β Lyræ -	5 21 39,62	22 10,50	22 39,62	23 10,50	23 41,25	5 22 40,18	5 51 5,30	
	Lyræ -	5 30 25,50	30 56,00	31 25,00	31 55,25	32 26,00	5 31 25,46	5 59 50,46	
	A Star -	5 38 48,75	39 19,50	39 48,75	40 18,50	40 49,12	5 39 48,89	6 8 13,77	
	κ Cygni -	5 50 38,25	——	52 2,25	——	——	5 51 20,25	6 19 45,00	Mean of 1st and mer. wires κ Cygni.
	β Cygni -	6 1 38,50	2 7,50	2 35,00	3 3,75	3 33,25	6 2 35,50	6 31 00,05	
	γ Aquilæ -	6 15 57,25	16 23,62	16 48,50	17 14,75	17 41,12	6 16 48,95	6 45 13,33	
	α Aquilæ -	6 20 14,50	20 41,00	21 5,62	21 31,62	21 57,75	6 21 6,02	6 49 30,32	
	β ——	6 24 42,25	25 8,50	25 33,37	25 59,12	26 25,00	6 25 33,60	6 53 57,84	
	γ Cygni -	6 53 38,25	54 12,00	54 43,75	55 16,87	55 50,00	6 54 44,10	7 23 7,92	

Transits of Stars at Rio de Janeiro continued. (1st Series.)

Date.	Stars.	1st Wire.	2nd Wire.	Mer. Wire.	4th Wire.	5th Wire.	Mean Chron.	Mean Clock.	
1822.		h. m. s.	m. s.	m. s.	m. s.	m. s.	h. m. s.	h. m. s.	
	1st Limb	11 2 52,20	3 18,00	3 43, 0	4 8,20	4 34,00			
Oct. 1	⊙'s 2d Limb	11 5 00,75	5 27,50	5 51,50	6 17,50	6 43,00			
	Centre	11 3 56,47	4 22,75	4 47,25	5 12,85	5 38,50	11 4 47,51	11 32 56,34	
							Clock at mean Noon	11 43 10,93	
P. M.	A Star -	5 34 34, 0	35 3,70	35 33, 0	36 4,00	36 34,00	5 35 33,62	6 3 36,02	
	ϰ Cygni -	5 46 24, 0	47 7,00	47 48,00	48 30,20	——	5 47 27,30	6 15 29,50	Mean of four first wires ϰ Cygni.
	Ditto	5 46 24, 0	——	47 48,00	——	——	5 47 6,00	6 15 8,20	Mean of 1st and mer. wires ϰ Cygni.
	β Cygni -	5 57 24, 0	57 53,00	58 20,50	58 49,00	59 18,00	5 58 20,92	6 26 22,97	
	γ Aquilæ -	6 11 42,50	12 9,00	12 33,50	12 59,50	13 26,00	6 12 34,00	6 40 35,77	
	α Aquilæ -	6 16 00, 0	16 26,00	16 51,00	17 17,00	17 43,00	6 16 51,33	6 44 53,05	
	β Aquilæ -	6 20 28,00	20 54,00	21 18,50	21 44,00	22 10,00	6 21 18,83	6 49 20,50	
	γ Cygni -	6 49 24,25	49 57,62	50 29,62	51 2,75	51 36,00	6 50 29,97	7 18 31,24	
	1st Limb	11 2 15,70	2 41,50	3 6,20	3 32,00	3 57,50			
2	⊙'s 2d Limb	11 4 25,00	4 50,50	5 14,50	5 40,00	6 6,50			
	Centre	11 3 20,35	3 46,00	4 10,35	4 36,25	5 2,00	11 4 10,88	11 31 56,55	
							Clock at mean Noon	11 42 30,21	
	1st Limb	11 0 57,00	1 22,50	1 47,50	2 13,25	2 39,00			
4	⊙'s 2d Limb	11 3 6,25	3 32,50	3 56,62	4 22,25	4 48,62			
	Centre	11 2 1,62	2 27,50	2 52,06	3 17,75	3 43,81	11 2 52,46	11 29 59,04	
							Clock at mean Noon	11 41 9,81	
	1st Limb	11 0 17,50	0 44,00	1 8,12	1 34,00	1 59,50			
5	⊙'s 2d Limb	11 2 27,00	2 53,12	3 17,50	3 43,25	4 9,12			
	Centre	11 1 22,25	1 48,56	2 12,81	2 38,63	3 4,31	11 2 13,23	11 29 00,83	
							Clock at mean Noon	11 40 29,65	
P. M.	α Lyræ -	4 47 42,25	48 15,50	48 46,62	49 19,50	49 52,50	4 48 47,16	5 15 30,11	
	γ Aquilæ -	5 54 37,75	55 4,00	55 29,12	55 55,00	56 21,12	5 55 29,35	6 22 11,25	
	α Aquilæ -	5 58 55,25	59 21,75	59 46,50	60 12,00	60 38,25	5 59 46,71	6 26 28,56	
	β Aquilæ -	6 3 24,00	3 49,00	4 14,00	4 39,50	5 5,00	6 4 14,25	6 30 56,05	
	γ Cygni -	6 32 19,00	32 53,00	33 24,62	33 57,75	34 31,12	6 33 25,02	7 00 6,37	
	α Lyræ -	——	——	——	49 19,50	49 52,50	4 49 36,00	5 16 18,95	Mean of 4th and 5th wires α Lyræ.

Observations of Coincidences at Rio de Janeiro. (1st *Series.*)

P. M. 28th Sept. 1822.
Clock losing at a mean rate $40^{s},58$. } Barom. 29,87.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
72,6	12 37 34	1,37						
	49 38	1,27	1,32	724			2,85	
	1 1 43	1,17	1,22	725			2,43	
	13 50	1,08	1,12	727			2,07	
	25 58	1,00	1,04	728			1,77	
	38 6	0,92	0,96	728			1,51	
	50 14	0,84	0,88	728			1,27	
	2 2 24	0,77	0,80	730			1,06	
72,4	14 35	0,71	0,74	731			0,89	
72,5				727,62	725,62	86122,05	1,73	86123,78
A.M 29th Sept.						Barom. 29,93.		
72,5	9 34 32	1,30						
	46 37	1,20	1,25	725			2,55	
	58 44	1,10	1,15	727			2,16	
	10 10 52	1,00	1,05	728			1,80	
	23 00	0,92	0,96	728			1,51	
	35 9	0,85	0,88	729			1,27	
	47 19	0,78	0,81	730			1,08	
	59 30	0,72	0,75	731			0,92	
72,8	11 11 41	0,68	0,70	731			0,80	
72,7				728,62	726,62	86122,37	1,51	86123,88
P. M. 29th September.						Barom. 29,87.		
73	12 42 54	1,36						
	54 59	1,25	1,30	725			2,78	
	1 7 5	1,16	1,20	726			2,37	
	19 11	1,06	1,11	726			2,01	
	31 19	0,97	1,01	728			1,68	
	43 27	0,90	0,93	728			1,42	
	55 37	0,83	0,86	730			1,22	
	2 7 47	0,76	0,79	730			1,03	
73	19 56	0,72	0,74	729			0,89	
73				727,75	725,75	86122,08	1,67	86123,75

A. M. September 30th. Barom. 29,90.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
	h. m. s.	°	°					
73,2	10 13 44	1,38						
	25 48	1,27	1,32	724			2,86	
	37 53	1,17	1,22	725			2,43	
	49 58	1,08	1,12	725			2,06	
	11 2 5	0,99	1,03	727			1,75	
	14 14	0,91	0,95	729			1,48	
	26 22	0,84	0,87	728			1,25	
	38 31	0,78	0,81	729			1,07	
74,0	50 40	0,72	0,75	729			0,92	
73,6				727	725	86121,84	1,73	86123,57

P. M. 30th September. Barom. 29,85.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
74	1 4 50	1,39						
	16 53	1,29	1,29	723			2,74	
	28 57	1,19	1,24	724			2,51	
	41 2	1,09	1,14	725			2,12	
	53 9	0,99	1,01	726			1,68	
	2 5 17	0,91	0,95	728			1,48	
	17 25	0,81	0,87	728			1,25	
	29 34	0,78	0,81	729			1,07	
74	41 43	0,73	0,75	729			0,93	
74				726,5	724,5	86121,68	1,72	86121,40

A. M. 1st October. Barom. 29,80.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
74,0	9 29 38	1,42						
	41 40	1,31	1,36	722			3,04	
	53 45	1,21	1,25	725			2,57	
	10 5 50	1,11	1,15	725			2,18	
	17 57	1,02	1,06	727			1,81	
	30 5	0,94	0,98	728			1,57	
	42 14	0,85	0,89	729			1,30	
	54 23	0,78	0,81	729			1,08	
74,5	11 6 32	0,74	0,76	729			0,94	
74,2				726,75	724,75	86121,76	1,81	86123,57

P. M. 1st Oct. Clock losing at a mean rate 40ˢ,58. Barom. 29,78.								
Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°						
74,6	1 2 40	1,36	°					
	14 44	1,25	1,30	724			2,78	
	26 49	1,15	1,20	725			2,35	
	38 55	1,06	1,10	726			2,00	
	51 0	0,98	1,02	725			1,70	
	2 3 7	0,90	0,94	727			1,44	
	15 15	0,83	0,86	728			1,22	
	27 24	0,77	0,80	729			1,05	
74,7	39 33	0,72	0,74	729			0,91	
74,7				726,62	724,62	86121,72	1,68	86123,40
2d October, A. M. Barom. 29,82.								
75	9 58 43	1,38						
	10 10 45	1,27	1,32	722			2,86	
	22 49	1,16	1,21	724			2,41	
	34 53	1,06	1,11	724			2,01	
	46 58	0,98	1,02	725			1,70	
	59 3	0,90	0,94	725			1,44	
	11 11 10	0,83	0,86	727			1,22	
	23 17	0,78	0,81	727			1,08	
75	35 25	0,73	0,75	728			0,93	
75				725,25	723,25	86121,27	1,71	86122,98
P. M. 2d October. Barom. 29,80.								
75	12 53 58	1,35						
	1 6 1	1,24	1,29	723			2,74	
	18 3	1,14	1,19	722			2,31	
	30 7	1,05	1,09	724			1,95	
	42 13	0,95	1,00	726			1,63	
	54 18	0,87	0,91	725			1,35	
	2 6 25	0,81	0,84	727			1,15	
	18 33	0,76	0,78	728			1,01	
75	30 40	0,71	0,73	727			0,88	
75				725,25	723,25	86121,27	1,63	86122,90

Temp. Fahrenheit.	Time of co-incidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
3d October, A. M.						Barom. 30,02.		
°	h. m. s.	°	°					
73	10 1 51	1,37						
	13 54	1,27	1,32	723			2,85	
	25 59	1,17	1,22	725			2,43	
	38 5	1,08	1,12	726			2,06	
	50 12	0,99	1,03	727			1,74	
	11 2 18	0,91	0,95	726			1,48	
	14 26	0,84	0,87	728			1,25	
	26 36	0,77	0,80	730			1,06	
73	38 45	0,71	0,74	729			0,89	
73				726,75	724,75	86121,76	1,72	86123,48
3d October, P. M.						Barom. 29,99.		
72,7	12 47 42	1,34						
	59 45	1,23	1,28	723			2,70	
	1 11 49	1,13	1,18	724			2,28	
	23 56	1,03	1,08	727			1,91	
	36 4	0,94	0,97	728			1,54	
	48 11	0,87	0,90	727			1,34	
	2 0 21	0,80	0,83	730			1,13	
	12 29	0,74	0,77	728			0,97	
72	24 39	0,69	0,71	730			0,84	
72,3				727,12	725,12	86121,88	1,59	86123,47
4th October, A. M.						Barom. 30,00.		
71,3	9 0 25	1,41						
	12 29	1,30	1,35	724			3,00	
	24 35	1,20	1,25	726			2,55	
	36 42	1,10	1,15	727			2,16	
	48 50	1,01	1,05	728			1,82	
	10 00 59	0,93	0,97	729			1,54	
	13 7	0,86	0,89	728			1,31	
	25 17	0,80	0,83	730			1,12	
71,8	37 25	0,74	0,77	728			0,97	
71,5				727,50	725,50	86122,01	1,81	86123,82

4th Oct. P. M. Clock losing at a mean rate $40^{s},58$. Barom. 29,96.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°						
72,2	12 1 45	1,38	°					
	13 50	1,28	1,33	725			2,89	
	25 55	1,18	1,25	725			2,55	
	38 2	1, 9	1,13	727			2,10	
	50 9	1, 0	1,04	727			1,78	
	1 2 17	0,92	0,96	728			1,51	
	14 26	0,85	0,88	729			1,28	
	26 35	0,79	0,82	729			1,10	
71,7	38 46	0,73	0,76	731			0,94	
72,0				727,62	725,62	86122,05	1,77	86123,82

5th October, A. M. Barom. 29,93.

71,2	9 11 45	1,37						
	23 51	1,27	1,32	726			2,85	
	35 58	1,17	1,22	727			2,43	
	48 6	1,08	1,12	728			2,07	
	10 0 15	0,99	1,03	729			1,75	
	12 24	0,91	0,95	729			1,48	
	24 33	0,83	0,87	729			1,24	
	36 44	0,77	0,80	731			1,05	
71,6	48 55	0,71	0,74	731			0,89	
71,4				728,75	726,75	86122,41	1,72	86124,13

5th October, P. M. Barom. 29,86.

72	12 10 50	1,37						
	22 54	1,27	1,32	724			2,85	
	35 00	1,16	1,21	726			2,41	
	47 07	1,07	1,11	727			2,03	
	59 15	0,98	1,02	728			1,72	
	11 23	0,90	0,94	728			1,44	
	23 32	0,82	0,86	729			1,21	
	35 42	0,77	0,79	730			1,03	
72,3	47 50	0,71	0,74	730			0,89	
72,2				727,75	725,75	86122,08	1,70	86123,78

TABLE I. (*1st Series.*)

Time by Clock of Transits of Stars at Rio de Janeiro.

Stars.	Time by clock, 29th Sept.	Time by clock, 30th.	Time by clock, 1st. Oct.	Time by clock, 5th.
	h. m. s.	h. m. s.	h. m. s.	h. m. s.
α Lyræ . . .	—	5 38 31,81	—	5 15 30,11
Do. 4th & 5th wires	5 43 56,91	39 20,79	—	5 16 18,95
β Lyræ . . .	5 55 42,34	51 5,30	—	—
γ Lyræ . . .	6 4 27,44	59 50,46	—	—
A Star.	—	6 8 13,77	6 3 36,02	—
κ Cygni 4 1st wires	6 24 43,90	—	6 15 29,50	—
Do. 1st and 3d wires	6 24 22,78	6 19 45,00	6 15 8,20	—
β Cygni . . .	—	6 31 0,05	6 26 22,97	—
γ Aquilæ . . .	6 49 49,95	6 45 13,33	6 40 35,77	6 22 11,25
α Aquilæ . . .	6 54 7,28	6 49 30,32	6 44 53,05	6 26 28,56
β Aquilæ . . .	—	6 53 57,84	6 49 20,50	6 30 56,05
γ Cygni . . .	—	7 23 7,92	7 18 31,24	7 00 06,37

TABLE II.

Transits of the Sun.

Time by Clock at the moment of mean Noon.

28th Sept.	29th.	30th.	1st Oct.	2nd.	4th.	5th.
h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
11 45 13,72	11 44 33,15	11 43 52,21	11 43 10,93	11 42 30,21	11 41 9,81	11 40 29,65

TABLE III. (1st Series.)

Rate of the Clock by the Stars Transits. (Losing.)

Stars.	From 29th to 30th Sept.	29th to 1st Oct.	29th to 5th.	30th to 1st Oct.	30th to 5th.	1st to 5th.
	s.	s.	s.	s.	s.	s.
α Lyræ	—	—	—	—	40,43	—
Do. 4th & 5th wires	40,21	—	40,42		—	—
β Lyræ	41,13	—	—		—	—
γ Lyræ	41,07	—	—		—	—
A Star . . .	—	—	—	41,84	—	—
κ Cygni 4 first wires	—	41,29	—		—	—
Do. 1st & 3d wires	41,87	—	—	40,89	—	—
β Cygni	—	—	—	41,17	—	—
γ Aquilæ . . .	40,71	41,18	40,54	41,65	40,51	40,22
α Aquilæ . . .	41,05	41,20	40,54	41,36	40,44	40,21
β Aquilæ . . .	—	—	—	41,43	40,45	40,20
γ Cygni	—	—	—	40,77	40,40	40,31
Mean by Stars . .	41,01	41,22	40,50	41,30	40,45	40,23
Proportion for rate in 3^m 56^s	+,11	+,11	+,11	+,11	+,11	+,11
Rate in a Mean Solar Day .	41,12	41,33	40,61	41,41	40,56	40,34

TABLE IV.

Rate by Sun's Transits. (Clock losing.)

28 to 29	28 to 30	28 to 1 Oct.	28 to 2	28 to 4	28 to 5	29 to 30	29 to 1	29 to 2	29 to 4	29 to 5	30 to 1	30 to 2	30 to 4	30 to 5	1 to 2	1 to 4	1 to 5	2 to 4	2 to 5	4 to 5
s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
40,57	40,75	40,93	40,88	40,65	40,58	40,94	41,11	40,98	40,67	40,58	41,28	41,0	40,60	40,51	40,72	40,37	40,32	40,20	40,19	40,16

TABLE V. (1*st. Series.*)

Vibrations of the Pendulum at Rio de Janeiro.
The clock making 86359,42 Vibrations in a mean solar day, at a mean rate.

Date.		Barom.	Therm.	Difference of Temperature from 68°	Vibrations in 24 hours.	Corrections for Temperature.	Vibrations. in 24 hours in Temp. 68.
1822.		Inches.	o				
Sept. 28	P. M.	29,87	72,5	4,5	86123,78	+1,90	86125,68
29	A. M.	29,93	72,7	4,7	86123,88	+1,99	86125,87
	P. M.	29,87	73,0	5,0	86123,75	+2,11	86125,86
30	A. M.	29,90	73,6	5,6	86123,57	+2,37	86125,94
	P. M.	29,85	74,0	6,0	86123,40	+2,54	86125,94
Oct. 1	A. M.	29,80	74,2	6,2	86123,57	+2,62	86126,19
	P. M.	29,78	74,7	6,7	86123,40	+2,83	86126,23
2	A. M.	29,82	75,0	7,0	86122,98	+2,96	86125,94
	P. M.	29,80	75,0	7,0	86122,90	+2,96	86125,86
3	A. M.	30,02	73,0	5,0	86123,48	+2,11	86125,59
	P. M.	29,99	72,3	4,3	86123,47	+1,82	86125,29
4	A. M.	30,00	71,5	3,5	86123,82	+1,48	86125,30
	P. M.	29,96	72,0	4,0	86123,82	+1,69	86125,51
5	A. M.	29,93	71,4	3,4	86124,13	+1,44	86125,57
	P. M.	29,86	72,2	4,2	86123,78	+1,78	86125,56
	Mean	29,89	73,1				86125,76

TABLE VI. (1*st. Series.*)

By the Stars.

From	To	Correct Vibrations in a mean solar day.	No. of Stars observed.	Interval of Seconds.	Sum of the Factors.
Sept. 1822.					
30th A. M.	30th P. M.	86125,40	5	1	6
	1st P. M.	86125,32	3	2	6
	5th P. M.	86125,71	3	6	18
1st A. M.	1st P. M.	86125,38	7	1	7
	5th P. M.	86125,72	5	5	25
2nd A. M.	5th P. M.	86125,82	4	4	16
Mean by Stars		86125,56	Sum of the Factors		78

TABLE VII. (1*st Series.*)

By the Sun.

From	To	Correct Vibrations in a mean solar day.	No. of Stars observed.	Interval of Transits.	Sum of the Factors.
Sept. 1822.					
28 P.M.	29 A.M.	86125,78	2	1	2
	30 A.M.	86125,67	2	2	4
	1 Oct. A.M.	86125,56	2	3	6
	2	86125,66	2	4	8
	4	86125,74	2	6	12
	5	86125,77	2	7	14
29 P.M.	30 A.M.	86125,54	2	1	2
	1 Oct. A.M.	86125,45	2	2	4
	2	86125,62	2	3	6
	4	86125,72	2	5	10
	5	86125,77	2	6	12
30 P.M.	1 Oct. A.M.	86125,36	2	1	2
	2	86125,65	2	2	4
	4	86125,77	2	4	8
	5	86125,81	2	5	10
1 Oct. P.M.	2 A.M.	86125,94	2	1	2
	4 A.M.	86125,91	2	3	6
	5	86125,92	2	4	8
2 P.M.	4 A.M.	86125,89	2	2	4
	5	86125,91	2	3	6
4 P.M.	5 A.M.	86125,96	2	1	2
Mean by Sun		86125,73	Sum of the Factors		132

Observations for the Latitude.

By the Sun.

15th October, 1822. Face East.		21st October 1822. Face West.	
Barometer 29,89. Thermometer 76°		Barometer 29,88. Thermometer 78°.	
	° ′ ″		° ′ ″
Readings { 1st Vernier .	14 20 15	Readings { 1st Vernier .	77 26 38
2nd ditto .	14 21 12	2nd ditto . .	77 27 23
	41 27		54 1
Observ^{d} M. Z.D. ⊙'s L. L.	14 20 43,5	Observed Mer. Altitude	77 27 0,5
Semidiameter .	— 16 4,9	⊙'s Semi Diameter . .	+ 16 6,5
Z. D. ⊙'s Centre . .	14 4 38,6	Apparent Alt. ⊙'s Centre	77 43 7
Refraction . .	+ 13,8	Refraction . .	— 0 12,3
Parallax . .	— 2,1	Parallax . .	+ 2,
⊙'s Mer. Z.D.	14 4 50,3	True Alt. of ⊙'s Centre	77 42 56,7
⊙'s Declination . . .	8 49 52,2 S.	⊙'s True Mer. Z. Dist.	12 17 3,3
Latitude, Face East .	22 54 42,5 S.	⊙'s Declination . . .	10 38 59,1 S.
		Latitude, Face West .	22 56 2,4 S.

Latitude, Face East . . .	22 54 42, 5
—— West . . .	22 56 2, 4
Latitude of Observatory .	22 55 22,45 South.

We have thus obtained 86125,56 vibrations by the stars, and 86125,73 by the sun; the sums of the factors being respectively 78 and 132, we may take 86125,66 as the final mean number of vibrations made by the pendulum in 24 hours.

The height of the pendulum above the level of low water was found by levelling 72 feet; the correction due to which is ,293; and as the ground beneath and immediately round the pendulum was granite, and sloping rapidly to the sea, it may be multiplied by $\frac{60}{100}$, which gives ,18 as the correction for the elevation.

The correction for buoyancy is 5,86, which added to ,18, gives 6,04 as the ultimate correction to be added to 86125,66; and thus we obtain 86131,70 for the number of vibrations this pendulum would have made in vacuo at the level of the sea, in temp. of 68° of FAHRENHEIT in a mean solar day at Rio de Janeiro in latitude 22° 55′ 22″, longitude 43° W.

From the foregoing data, and the number of vibrations made at the level of the sea in London by the same pendulum, on the return to England,* viz. 86236,95, and 39,13929 inches the length of the second's pendulum in London, we arrive at the length of the second's pendulum at Rio de Janeiro 39,04381 inches, whence the following results are deduced.

Stations compared with Rio de Janeiro, in Latitude 22° 55′ 22″ S.	Diminution of Gravity from the Pole to the Equator.	Ellipticity.	Length of Equat. Pend.
Unst . . in Lat. 60° 45′ 28″ N.	,0053671	$\frac{1}{304,55}$	39,01204
Portsoy 57 40 59	,0053672	$\frac{1}{304,57}$	,01204
Leith Fort 55 58 41	,0053508	$\frac{1}{303,06}$	,01214
Clifton 53 27 43	,0053042	$\frac{1}{298,84}$	,01242
Arbury Hill . . . 52 12 55	,0053495	$\frac{1}{302,94}$	,01215
London 51 31 8	,0053079	$\frac{1}{299,16}$	,01240
Shanklin Farm . . 50 37 24	,0053087	$\frac{1}{299,24}$	,01239
Mean	,0053365	$\frac{1}{301,77}$	39,01223

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* See Remarks after the Experiment, in the Appendix.

Experiment No. VI. *Second Series, at Rio de Janeiro. By Mr.* HENRY FOSTER.

Transits observed at Rio de Janeiro.

Date.	Stars.	1st Wire.	2nd Wire.	3rd Wire.	4th Wire.	5th Wire.	Mean Chronometer.	Clock.
1822.		h. m. s.	h. m. s.	h. m. s.	h. m s.	m. s.	h. m. s.	h. m. s.
Oct. 22	α Cygni -	5 38 55,25	39 32,75	40 6, 0	40 42,75	41 19, 0	5 40 6,96	5 0 24, 0
	ξ Cygni -	6 1 59,50	2 34,75	3 8,50	3 43,25	4 19, 0	6 3 8,92	5 23 25,60
	Ditto -	6 9 4,50	9 33,75	10 2,50	10 31,40	11 0,50	6 10 2,52	5 30 19,10
	e Pegasi -	6 17 36,75	18 3,50	18 30,25	18 57,50	19 24,50	6 18 30,46	5 38 46,91
	ditto ——	——	——	18 30,25	18 57,50	19 24,50	6 18 57,42	5 39 13,87
	ι ——	6 39 11,25	39 37, 0	40 1,25	40 28, 0	40 53,50	6 40 2.04	6 00 18,13
	t ——	7 2 20, 0	2 48,50	3 15, 0	3 43,25	4 11,75	7 3 15,58	6 23 31,30
	η ——	——	7 38 36,50	39 5, 0	39 34,75	40 4,25	7 39 20,12	6 59 35,30
	μ ——	——	——	——	7 46 16,50	——	7 46 16,50	7 6 31,55
	⊙'s 1st Limb	10 49 57, 0	50 23,25	50 49, 0	51 15, 0	51 41, 0		
23	⊙'s 2d Limb	10 52 9, 0	52 34, 0	52 59, 0	53 26, 0	53 52,25		
	⊙'s Centre	10 51 3, 0	51 28,62	51 54, 0	52 20,50	52 46,62	10 51 54,46	10 11 56,83
							Clock at mean Noon =	10 27 26,78
P.M.	α Cygni -	5 34 39,50	35 15, 0	35 49,50	36 25,70	37 1,50	5 35 50,12	4 55 47,04
	ζ ——	6 4 46,50	5 16, 0	5 44,80	6 14, 0	6 43, 0	6 5 44,85	5 25 41,31
	e Pegasi -	——	——	6 14 12,50	14 39,50	15 6,50	6 14 39,50	5 34 35,83
	ι ——	6 34 53, 0	35 19,50	35 44,50	36 10,50	36 36,50	6 35 44,75	5 55 40,76
	t ——	6 58 1,50	58 30, 0	58 57, 0	59 25, 0	59 53,50	6 58 57,33	6 18 52,99
	η ——	——	7 34 19,50	34 47,50	35 18, 0	35 47, 0	7 35 3, 0	6 54 57,96
	μ ——	——	——	——	7 41 59,50	——	7 41 59,50	7 1 54,36
	Ditto	——	——	7 41 31,50	41 59,50	——	7 41 45,50	7 1 40,36
	β ——	7 54 16,50	54 45,50	55 13, 0	55 42, 0	56 10, 0	7 55 13,33	7 15 7,99
	⊙'s 1st Limb	10 49 29, 0	49 55,20	50 20, 0	50 46,50	51 13, 0		
24	⊙'s 2d Limb	10 51 41, 0	52 7,50	52 32,50	52 58,50	53 25, 0		
	⊙'s Centre	10 50 35, 0	51 1,35	51 26,25	51 52,50	52 19, 0	10 51 26,72	10 11 7,17
							Clock at mean Noon =	10 26 44,94
	⊙'s 1st Limb	10 48 32, 0	48 59, 0	49 24,20	49 50, 0	50 16,50		
26	⊙'s 2d Limb	10 50 45,50	51 11,80	51 37, 0	52 3, 0	52 29,50		
	⊙'s Centre	10 49 38,75	50 5,40	50 30,60	50 56,50	51 23, 0	10 50 30,81	10 9 30,48
							Clock at mean Noon =	10 25 21,88

Transits observed at Rio de Janeiro. (2nd Series.)

Date.	Stars.	1st Wire.	2nd Wire.	3rd Wire.	4th Wire.	5th Wire.	Mean Chronometer.	Clock.
1822.		h. m. s.	h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.	h. m. s.
Oct. 28	⊙'s 1st Limb	10 47 35, 0	48 2, 0	48 27, 0	48 53,20	49 19,80		
	2d Limb	10 49 48, 0	50 14,70	50 40, 0	51 6, 0	51 32,50		
	Centre	10 48 41,50	49 8,35	49 33 50	49 59,60	50 26,15	10 49 33,76	10 7 56,76
							Clock at mean Noon =	10 23 58,88
29	⊙'s 1st Limb	10 47 6,50	47 33,50	47 59, 0	48 25, 0	48 51,20		
	2d Limb	10 49 20,50	49 47, 0	50 12, 0	50 38,20	51 5, 0		
	Centre	10 48 13,50	48 40,25	49 5,50	49 31,60	49 58,10	10 49 5,76	10 7 10,35
							Clock at mean Noon =	10 23 16,68
30	⊙'s 1st Limb	10 46 40, 0	47 7, 0	47 32,80	47 59, 0	48 25, 0		
	2d Limb	10 48 54, 0	49 20,50	49 46, 0	50 12,50	50 39, 0		
	Centre	10 47 47, 0	48 13,75	48 39,40	49 5,75	49 32, 0	10 48 39,55	10 6 25,32
							Clock at mean Noon =	10 22 35,16
31	⊙'s 1st Limb	10 46 13, 0	46 40,50	47 5,80	47 32, 0	47 58,20		
	2d Limb	10 48 27,50	48 54,50	49 19,50	49 46, 0	50 12,80		
	Centre	10 47 20,25	47 47,50	48 12,65	48 39, 0	49 5,50	10 48 12,92	10 5 40,13
							Clock at mean Noon =	10 21 52,67
Nov. 1	⊙'s 1st Limb	10 45 48,50	46 15.50	46 40,50	47 7, 0	47 33,50		
	2d Limb	10 48 2,50	48 29, 0	48 54,50	49 21, 0	49 47,50		
	Centre	10 46 55,50	47 22,25	47 47,50	48 14, 0	48 40,50	10 47 47,87	10 4 56,07
							Clock at mean Noon =	10 21 10,51
P. M.	α Cygni -	4 55 53, 0	56 29,75	57 3,75	57 40,25	58 16, 0	4 57 4,42	4 14 7,71
	ξ ——	5 18 56,75	19 32,25	20 5,50	20 41, 0	21 16,25	5 20 6,21	4 37 9,14
	ζ ——	5 26 1,25	26 31, 0	26 59,25	27 29, 0	27 58,50	5 26 59,71	4 44 2,54
	e Pegasi -	5 34 34,50	35 1,75	35 27,50	35 55, 0	36 21,75	5 35 28, 0	4 52 30,71
	Ditto	——	——	35 27,50	35 55, 0	36 21,75	——	4 52 57,46
	ι ——	5 56 7,50	56 34,50	56 59,50	57 25,50	57 51,50	5 56 59,67	5 14 2,05
	t ——	6 19 17, 0	19 45,20	20 12, 0	20 40, 0	21 8,50	6 20 12,45	5 37 14,59
2	⊙'s 1st Limb	10 45 25,80	45 52,50	46 17,80	46 44,50	47 11, 0		
	2d Limb	10 47 40, 0	48 6,50	48 32, 0	48 58,50	49 25,20		
	Centre	10 46 32,90	46 59,50	47 24,90	47 51,50	48 18,10	10 47 25,30	10 4 13,06
							Clock at mean Noon =	10 20 28,61
P. M.	μ Pegasi -	——	——	——	6 58 57,50	——	6 58 57,50	6 15 37,79
	——	——	——	6 58 29,50	58 57,50	——	6 58 43,50	6 15 23,79
	β ——	7 11 15, 0	11 44, 0	12 11,50	12 40,50	13 9, 0	7 12 11,92	6 28 52, 0

Comparisons of Clock with Chronometer 438 at Rio de Janeiro.

(2nd Series.)

Date.		Chronometer.	Clock.	Difference.
				Clock slow.
		h. m. s.	h. m. s.	h. m. s.
October 22, 1822,	P.M.	5 9 42,5	4 30 0	0 39 42,5
	——	6 45 44,0	6 6 0	0 39 44,0
	——	10 1 47,0	9 22 0	0 39 47,0
23,	Noon	10 9 57,0	9 30 0	0 39 57,0
	P.M.	5 1 2,5	4 21 0	0 40 2,5
	——	7 5 4·5	6 25 0	0 40 4,5
	——	8 33 6,0	7 53 0	0 40 6,0
24,	Noon	11 21 20	10 41 0	0 40 20,0
26,	Noon	11 4 0,5	10 23 0	0 41 0,5
28,	Noon	10 41 36,9	10 00 0	0 41 36,9
29,	Noon	10 55 55,5	10 14 0	0 41 55,5
30,	Noon	10 31 14,0	9 49 0	0 42 14,0
31	Noon	10 25 32,5	9 43 0	0 42 32,5
November 1,	Noon	10 25 51,5	9 43 0	0 42 51,5
	P M.	5 48 57,5	5 6 0	0 42 57,5
	——	9 44 1,0	9 1 0	0 43 1,0
2,	Noon	11 6 12,5	10 23 0	0 43 12,5
	P.M.	4 20 17,0	3 37 0	0 43 17,0
	——	7 17 20,0	6 34 0	0 43 20,0

TABLE I.

Times by Clock at Transits of Stars,

Stars.	October 22nd.	23rd.	November 1st.	2nd.
	h. m. s.	h. m. s.	h. m. s.	h. m. s.
α Cygni	5 0 24, 0	4 55 47,04	4 14 7,71	——
ξ ——	5 23 25,60	——	4 37 9,14	——
ζ ——	5 30 19,10	5 25 41,31	4 44 2,54	——
e Pegasi . . .	5 38 46,91	——	4 52 30,71	——
e Pegasi, 3rd, 4th, and 5th wires	5 39 13,87	5 34 35,83	4 52 57,46	——
ε —— . . .	6 0 18,13	5 55 40,76	5 14 2,05	——
t —— . . .	6 23 31,30	6 18 52,99	5 37 14,59	——
η ——	6 59 35,30	6 54 57,96	——	——
μ —— 4th wire . .	7 6 31,55	7 1 54,36	——	6 15 37,79
μ —— 4th and 5th wires .	——	7 1 40,36	——	6 15 23,79
β ——	——	7 15 7,99	——	6 28 52, 0

TABLE II.

Transits of Sun at Rio de Janeiro.

Time by Clock at mean Noon Oct. 23rd.	24th.	26th.	28th.	29th.	30th.	31st.	Nov. 1st.	2nd.
h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
10 27 26,78	10 26 44,94	10 25 21,83	10 23 58,88	10 23 16,68	10 22 35,16	10 21 52,67	10 21 10,51	10 20 28,61

TABLE III. *(2nd Series.)*

Rate of the Clock by the Stars—(*losing.*)

Stars.	From 22d Oct. to 23d.	From 22d to 1 Nov.	22nd to 2nd	23d to 1st.	23rd to 2nd.
	s.	s.	s.	s.	s.
α Cygni . . .	41,05	41,72	——	41,79	——
ξ —— . . .	——	41,74	——	——	——
ζ —— . . .	41,88	41,75	——	41,73	——
e Pegasi . . .	——	41,71	——	——	——
e —— 3d 4th and 5th wires	42,13	——	——	41,69	——
ε —— . . .	41,46	41,70	——	41,72	——
t —— . . .	42,40	41,76	——	41,69	——
η —— 2d, 3d, 4th, and 5th wires . .	41,43	——	——	——	——
μ —— 4th wire	41,28	——	41,71	——	——
μ —— 3d and 4th wire .	——	——	——	——	41,75
β —— . . .	——	——	——	——	41,69
Mean of Rates .	41,66	41,73	41,71	41,72	41,72
Proportion of Rate for $3^m 56^s$	+ ,11	+ ,11	+ ,11	+ ,11	+ ,11
Rate of clock in a mean solar day	41,77	41,84	41,82	41,83	41,83
Time to which the above Rates are due . .	A. M. 23d	P.M. 27th	A. M. 28	A.M. 28th	P.M. 28th

TABLE IV.

Rate of the Clock by the Sun—(*losing.*)

From 23rd. to 24th.	23rd. to 26th.	23rd. to 28th.	23rd. to 29th.	23rd. to 30th.	23rd. to 31st.	23rd. to 1st.	23rd. to 2nd.	24th. to 26th.	24th. to 28th.	24th. to 29th.	24th. to 30th.	24th. to 31st.	24th. to 1st.	24th. to 2nd.	26th. to 28th.	26th. to 29th.	26th. to 30th.
s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s,	s.	s.	s.	s.
41,84	41,63	41,58	41,68	41,66	41,76	41,81	41,82	41,53	41,51	41,65	41,63	41,75	41,80	41,81	41,50	41,73	41,68

26th. to 31st.	26th. to 1st.	25th. to 2nd.	28th. to 29th.	28th. to 30th.	28th. to 31st.	28th. to 1st.	28th. to 2nd.	29th. to 30th.	29th. to 31st	29th. to 1st.	29th. to 2nd.	30th. to 31st.	30th. to 1st.	30th. to 2nd.	31st. to 1st.	31st. to 2nd.	1st. to 2nd.
s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
41,84	41,89	41,90	42,20	41,86	42,07	42,09	42,05	41,52	42,00	42,06	42,02	42,49	42,32	42,18	42,16	42,03	41,90

Observations of Coincidences at Rio de Janeiro. (*2nd Series.*)

Clock losing 41^{s},82 at a mean ràte. Oct. 23, A. M. Barom. 29,78.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of Clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
73	8 12 41	1,35						
	24 46	1,25	1,30	725			2,76	
	36 53	1,15	1,20	727			2,35	
	49 00	1,05	1,10	727			1,98	
	9 1 8	0,96	1,005	728			1,64	
	13 16	0,89	0,925	728			1,40	
	25 25	0,82	0,855	729			1,19	
	37 34	0,75	0,785	729			1,01	
74,3	49 44	0,71	0,730	730			0,87	
73,6				727,87	725,87	86120,89	1,65	86122,54
P. M. October 23.						Barom. 29,71.		
76	1 16 47	1,34						
	28 50	1,23	1,285	723			2,70	
	40 53	1,14	1,185	723			2,29	
	52 57	1,05	1,095	724			1,96	
	2 5 3	0,95	1,000	726			1,63	
	17 10	0,88	0,915	727			1,37	
	29 16	0,82	0,850	726			1,18	
	41 24	0,76	,790	728			1,02	
76	53 32	0,70	,730	728			0,87	
76				725,62	723,62	86120,15	1,63	86121,78
A. M. October 24.						Barom. 29,74.		
77	8 12 5	1,31						
	24 8	1,20	1,255	723			2,57	
	36 12	1,10	1,15	724			2,16	
78,1	48 16	1,02	1,06	724			1,84	
	9 0 22	0,95	0,985	726			1,58	
	12 27	0,87	0,91	725			1,35	
	24 34	0,81	0,84	727			1,15	
	36 40	0,75	0,78	726			0,99	
79	48 46	0,70	0,725	726			0,86	
78				725,12	723,12	86119,99	1,56	86121,55

Temp. Fahrenheit.	Time of coincidence	Arc of Vibration.	Mean Arc.	Interval in Seconds of Clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
Clock losing $41^s,82$ at a mean rate. Oct. 24, P.M. Barom. 29,73.								
°	h. m. s.	°						
78,5	11 35 32	1,39	°					
	47 32	1,28	1,335	720			2,91	
	59 33	1,18	1,230	721			2,47	
	12 11 35	1,08	1,13	722			2,09	
	23 39	1,00	1,04	724			1,77	
78	35 43	0,92	0,96	724			1,51	
	47 47	0,85	0,885	724			1,28	
	59 52	0,79	0,820	725			1,10	
78	1 11 58	0,72	0,755	726			0,93	
78,2				723,25	721,25	86119,37	1,76	86121,13
Clock losing $41^s,82$. October 25, A. M. Barom. 29,88.								
76	8 20 00	1,34						
	32 2	1,23	1,285	722			2,70	
	44 6	1,13	1,180	724			2,28	
76,2	56 10	1,03	1,080	724			1,91	
	9 8 14	0,95	0,990	724			1,60	
	20 22	0,88	,915	728			1,37	
	32 28	0,81	,845	726			1,16	
	44 36	0,74	,775	728			0,98	
76,7	56 42	0,69	,715	726			0,83	
76,4				725,25	723,25	86120,03	1,60	86121,63
P. M. October 25. Barom. 29,88.								
77	11 18 20	1,33						
	30 22	1,22	1,275	722			2,66	
	42 25	1,12	1 170	723			2,24	
76,4	54 28	1,02	1,020	723			1,70	
	12 6 34	0,94	0,98	726			1,57	
	18 40	0,88	0,91	726			1,35	
	30 46	0,82	0,85	726			1,18	
	42 52	0,76	0,79	726			1,02	
76,2	55 9	0,70	0,73	727			0,87	
76,5				724,87	722,87	86119,91	1,57	86121,48

Clock losing 41^{s},82 at a mean rate. Oct. 26, A. M. Barom. 29,90.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of Clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
76	8 42 20	1,43						
	54 21	1,32	1,375	721			3,09	
	9 6 23	1,21	1,265	722			2,61	
	8 28	1,11	1,160	725			2,20	
76,2	30 34	1,02	1,065	726			1,85	
	42 39	0,93	0,975	725			1,55	
	54 44	0,85	0,890	725			1,29	
	10 6 53	0,79	0,820	729			1,10	
76,5	19 2	0,74	0,765	729			0,95	
76,2				725,25	723,25	86120,03	1,83	86121,86
P. M. October 26.						Barom. 29,89.		
76	11 25 13	1,36						
	37 17	1,25	1,305	724			278	
	49 20	1,15	1,20	723			2,35	
	12 1 23	1,05	1,10	723			1,98	
76,5	13 29	0,96	1,005	726			1,65	
	25 36	0,89	0,925	727			1,40	
	37 43	0,82	0,855	727			1,19	
	49 50	0,77	0,795	727			1,03	
76,5	1 1 56	0,72	0,745	726			0,91	
76,3				725,37	723,37	86120,07	1,66	86121,73
October 27, A. M.						Barom. 29,90.		
73,9	8 6 57	1,34						
	19 3	1,23	1,285	726			2,70	
	31 9	1,13	1,180	726			2,28	
	43 16	1,03	1,080	727			1,91	
74,2	55 26	0,95	0,990	730			1,60	
	9 7 34	0,88	0,915	728			1,37	
	19 46	0,81	0,845	732			1,16	
	31 56	0,75	0,780	730			0,99	
74,5	44 8	0,70	0,725	732			0,86	
74,2				728,87	726,87	86121,21	1,66	86122,82

Clock losing $41^s,82$ at a mean rate. Oct. 27, P.M. Barom. 29,82.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of Clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°						
74,9	11 17 4	1,33	°					
	29 9	1,22	1,275	725			2,66	
	41 15	1,13	1,175	726			2,26	
	53 22	1,04	1,085	727			1,92	
74,9	12 5 28	0,95	0,975	726			1,55	
	17 36	0,88	,915	728			1,37	
	29 45	0,82	,850	729			1,18	
	41 56	0,75	,785	731			1,01	
74,8	54 5	0,70	,725	729			0,86	
74,9				727,62	725,62	86120,81	1,60	86122,41
October 28, A. M.						Barom. 29,79.		
74,5	8 13 15	1,36						
	25 20	1,25	1,305	725			2,78	
	37 26	1,15	1,200	726			2,35	
	49 32	1,06	1,105	726			1,99	
75,2	9 1 40	,98	1,020	728			1,70	
	13 48	,91	0,945	728			1,46	
	25 56	,84	0,875	728			1,25	
	38 8	,78	0,810	732			1,07	
75,5	50 16	,72	0,750	728			0,92	
75,1				727,62	725,62	86120,81	1,69	86122,50
P. M. October 28.						Barom. 29,76.		
75,9	11 13 48	1,36						
	25 53	1,25	1,305	725			2,78	
	37 59	1,15	1,200	726			2,35	
	50 5	1,05	1,100	726			1,98	
75,8	12 2 12	0,96	1,005	727			1,65	
	14 20	0,89	0,925	728			1,40	
	26 28	0,82	0,855	728			1,19	
	38 38	0,75	7 85	730			1,01	
75,5	50 48	0,69	,720	730			0,85	
75,7				727,50	725,50	86120,77	1,65	86122,42

Clock losing 41^{s},82 at a mean rate. Oct. 29, A.M. Barom. 29,77.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of Clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°						
74,7	7 47 45	1,33	°					
	59 52	1,22	1,275	727			2,66	
	8 11 58	1,12	1,170	726			2,44	
	24 6	1,03	1,075	728			1,89	
75	36 15	0,94	0,985	729			1,59	
	48 25	0,86	,900	730			1,32	
	9 00 34	0,79	,825	729			1,11	
	12 46	0,73	,760	732			0,94	
75,2	24 56	0,67	,700	730			,80	
75				728,87	726,87	86121,21	1,57	86122,78
P.M. October 29.						Barom. 29,77.		
75,1	11 19 12	1,33						
	31 17	1,22	1,275	725			2,66	
	43 24	1,12	1,170	727			2,24	
	55 31	1,03	1,075	727			1,89	
75,5	12 7 39	,94	0,985	728			1,59	
	19 48	,86	,900	729			1,32	
	31 58	,80	,830	730			1,12	
	44 8	,73	,765	730			0,95	
75,8	56 20	,68	,705	732			0,81	
75,5				728,50	726,50	86121,10	1,57	86122,67
October 30, A.M.						Barom. 29,82.		
75	8 16 36	1,39						
	28 41	1,28	1,335	725			2,91	
	40 47	1,18	1,230	726			2,47	
	52 54	1,09	1,135	727			2,10	
76	9 5 2	1,00	1,045	728			1,78	
	17 10	0,92	0,960	728			1,51	
	29 20	,84	,880	730			1,27	
	41 30	,78	,810	730			1,07	
76,8	53 40	,72	,750	730			0,92	
75,9				728,	726,	86120,87	1,75	86122,62

Clock losing 41^{s},82 at a mean rate. Oct. 30, P. M. Barom. 29,82.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
76,5	1 16 56	1,38						
	28 59	1,27	1,325	723			2,87	
	41 3	1,17	1,220	724			2,43	
	53 10	1,07	1,120	727			2,05	
76,2	2 5 17	0,98	1,025	727			1,72	
	17 25	0,90	0, 94	727			1,44	
	29 33	,82	0, 86	728			1,21	
	41 42	,75	0,785	729			1,01	
76	53 54	,70	0,725	732			0,86	
76,2				727,25	725,25	86120,69	1,70	86122,39

October 31, A. M. Barom. 29,89.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
76	8 7 1	1,35						
	19 5	1,24	1,295	724			2,74	
	31 11	1,14	1,190	726			2,31	
	43 17	1,05	1,095	726			1,96	
76,1	55 25	0,97	1,010	728			1,67	
	9 7 33	,89	0,930	728			1,41	
	19 42	,82	,855	729			1,19	
	31 52	,76	,790	730			1,02	
76,2	44 2	,70	,730	730			0,87	
76,1				727,62	725,62	86120,81	1,65	86122,46

P. M. October 31. Barom. 29,87.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
76,6	11 18 54	1,36						
	30 58	1,25	1,305	724			2,78	
	43 3	1,15	1,200	725			2,35	
	55 10	1,05	1,100	727			1,98	
76,5	12 7 18	0,97	1,010	728			1,67	
	19 26	,89	0,930	728			1,41	
	31 35	,82	,855	729			1,19	
	43 45	,76	,790	730			1,02	
76,5	55 55	,70	,730	730			0,87	
76,5				727,62	725,62	86120,81	1,66	86122,47

Clock losing 41^s,82 at a mean rate. Nov. 1, A. M. Barom 29,82.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of Clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
75,5	8 15 26	1,39						
	27 30	1,28	1,335	724			2,91	
	39 35	1 18	1,230	725			2,47	
	51 41	1,09	1,135	726			2,10	
76,4	9 3 47	1,00	1,045	726			1,78	
	15 54	0,92	0,960	727			1,51	
	28 3	,84	,880	729			1,27	
	40 12	,77	,805	729			1,06	
76,5	52 22	,72	,745	730			0,91	
76,1				727,	725,	86120,61	1,75	86122,36
P. M. November 1.						Barom. 29,74.		
77	11 52 47	1,41						
	12 4 49	1,30	1,355	722			3,00	
	16 52	1,20	1,250	723			2,55	
	28 56	1,11	1,145	724			2,14	
77,2	41 2	1,02	1,065	726			1,85	
	53 9	0,93	0,975	727			1,55	
	1 5 14	,86	,895	725			1,31	
	17 21	,80	,830	727			1,12	
77,2	29 28	,75	,775	727			0,98	
77,1				725,12	723,12	86119,99	1,81	86121,80.
November 2, A.M.						Barom. 29,71.		
77,8	8 8 42	1,34						
	20 45	1,23	1,285	723			2,70	
	32 49	1,13	1,180	724			2,28	
	44 52	1,03	1,080	723			1,91	
78,8	56 57	0,94	0,985	725			1,59	
	9 9 2	,87	,905	725			1,34	
	21 8	,81	,840	726			1,15	
	33 14	,75	,780	726			0,99	
78,9	45 22	,70	,725	728			,86	
78,5				725,	723,	86119,95	1,60	86121,55

Clock losing $41^s,82$ at a mean rate. Nov. 2, P. M. Barom. 29,69.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds of Clock.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.							
78,5	11 2 55	1,39						
	14 55	1,28	1,335	720			2,91	
	26 57	1,18	1,230	722			2,47	
	39 1	1,09	1,135	724			2,10	
79,	51 5	1,00	1,045	724			1,78	
	12 3 10	0,92	0,960	725			1,51	
	15 15	,85	,885	725			1,28	
	27 20	,78	,815	725			1,08	
79,9	39 25	,72	,750	725			0,92	
79,1				723,75	721,75	86119,54	1,76	86121,30

TABLE V. *(2nd Series.)*

Vibrations of the Pendulum at Rio de Janeiro,
the Clock making 86358,18 Vibrations in a mean Solar Day.

Date.	Barometer.	Thermometer.	Difference of Temperature and 68°	Vibrations of Pendulum in 24 hours, corrected for Arc.	Correction for Temperature.	Vibrations in 24 hours at Temperature 68 degrees.
October	Inches.	°				
23 A. M.	29,78	73 6	5,6	86122,54	+ 2,37	86124,91
P. M.	29,71	76 0	8,0	86121,78	+ 3,38	86125,16
24 A. M.	29,74	78 0	10,0	86121,55	+ 4,23	86125,78
P. M.	29,73	78,2	10,2	86121,13	+ 4,31	86125,44
25 A. M.	29,88	76,4	8,4	86121,63	+ 3,55	86125,18
P. M.	29,88	76,5	8,5	86121,48	+ 3,59	86125,07
26 A. M.	29,90	76,2	8,2	86121,86	+ 3,47	86125,33
P. M.	29,89	76,3	8,3	86121,73	+ 3,51	86125,24
27 A. M.	29,90	74,2	6,2	86122,82	+ 2,62	86125,44
P. M.	29,82	74,9	6,9	86122,41	+ 2,92	86125,33
28 A. M.	29,79	75,1	7,1	86122,50	+ 3,00	86125,50
P. M.	29,76	75,7	7,7	86122,42	+ 3,26	86125,68
29 A. M.	29,77	75,0	7,0	86122,78	+ 2,96	86125,74
P. M.	29,77	75,5	7,5	86122,67	+ 3,17	86125,84
30 A. M.	29,82	75,9	7,9	86122,62	+ 3,34	86125,96
P. M.	29,82	76,2	8,2	86122,39	+ 3,47	86125,86
31 A. M.	29,89	76,1	8,1	86122,46	+ 3,43	86125,89
P. M.	29,87	76,5	8,5	86122,47	+ 3,59	86126,06
Nov. 1 A. M.	29,82	76,1	8,1	86122,36	+ 3,43	86125,79
P. M.	29,74	77,1	9,1	86121,80	+ 3,85	86125,65
A. M.	29,71	78,5	10,5	86121,55	+ 4,44	86125,99
P. M.	29,69	79,1	11,1	86121,30	+ 469	86125,99
Mean	29,80	76,2			Mean	86125,53

TABLE VI. (*2nd Series.*)

By the Stars.

	Correct Vibrations in a mean solar day.	No. of Stars obser-ved.	Interval of Transits.	Sum of Factors.
Oct. 23 A.M. to 23rd P. M.	86125,08	7	1	7
Nov. 1st P. M.	86125,52	6	10	60
— 2d P. M.	86125,58	1	11	11
24 A. M. to 1st P. M.	86125,59	5	10	50
2d P. M.	86125,63	2	10	20
	86125,48			148

TABLE VII. (*2nd. Series.*)

By the Sun.

	Correct Vibrations in a mean solar day.	No. of Stars obser-ved.	Interval of Transits.	Sum of Factors.
From Oct. 23d P.M. to 24th A. M.	86125,45	2	1	2
— 26th A. M.	86125,52	2	3	6
— 28th A. M.	86125,59	2	5	10
— 29th A. M.	86125,55	2	6	12
— 30th A. M.	86125,64	2	7	14
— 31st A. M.	86125,59	2	8	16
— Nov. 1st A. M.	86125,58	2	9	18
— 2d A. M.	86125,60	2	10	20
From — 24th P.M. to 26th A. M.	86125,54	2	2	4
— 28th A. M.	86125,63	2	4	8
— 29th A. M	86125,56	2	5	10
— 30th A. M.	86125,67	2	6	12
— 31st A. M.	86125,60	2	7	14
— Nov. 1st A. M.	86125,60	2	8	16
— 2d	86125,62	2	9	18
From — 26th P. M. to 28th A. M.	86125,70	2	2	4
— 29th A. M.	86125,58	2	3	6
— 30th A. M.	86125,73	2	4	8
— 31st A. M.	86125,63	2	5	10
— Nov. 1st A. M.	86125,62	2	6	12
2d A. M.	86125,63	2	7	14
From — 28th P. M. to 29th A. M.	86125,33	2	1	2
— 30th A. M.	86125,76	2	2	4
— 31st A. M.	86125,58	2	3	6
— Nov. 1st A. M.	86125,58	2	4	8
— 2d A. M.	86125,62	2	5	10
From — 29th P. M. to 30th A. M.	86126,20	2	1	2
— 31st A. M.	86125,71	2	2	4
— Nov. 1st A. M.	86125,66	2	3	6
— 2d A, M.	86125,68	2	4	8
From — 30th P. M. to 31st A. M.	86125,20	2	1	2
— Nov. 1st A. M.	86125,37	2	2	4
— 2d A. M.	86125,50	2	3	6
From — 31st P. M. to Nov. 1st A. M.	86125,58	2	1	2
— 2d A. M.	86125,66	2	2	4
From Nov. 1 P. M. to Nov. 2d A. M.	86125,74	2	1	2
Mean	86125,61			304

Thus we have obtained 86125,61 vibrations made by the pendulum by the transits of the sun, and by the stars 86125,48.

But the sum of the factors for the sun being 304, and that for the stars only 148, we have 0,04 to subtract from the vibrations given by the sun to arrive at 86125,57, the mean number of vibrations made by the pendulum in 24 hours.

The ball of the pendulum was elevated above the level of the sea 72 feet, the correction for which is ,293; but from the nature of the ground on which the pendulum stood, this requires to be multiplied by $\frac{6}{10}$ to obtain the true correction due to this elevation, or +0,18.

The correction for the buoyancy of the atmosphere is +5,80, to which must be added 0,18, and we obtain 5,98 for the final correction to be added to 86125,57, the mean number of vibrations made in 24 hours; and thus we arrive at 86131,55, for the number of vibrations that would be made by the pendulum in vacuo at the level of the sea and temperature 68° at Rio de Janeiro in latitude 22° 55′ 22″ south, and longitude 43¼° west from Greenwich.

From the above data, with the number of this pendulum's vibrations determined in London on the return,* and the length of the seconds' pendulum there, the length of the pendulum vibrating seconds at Rio de Janeiro appears to be 39,04368 inches; and comparing this with the lengths ascertained at different places by Captain Kater, we obtain the following ellipticities.

Stations compared with Rio de Janeiro in Lat. 22° 55′ 22″ S.	Diminution of Gravity from Pole to Equator	Ellipticity.	Length of Equatorial Pendulum.
Unst . in Lat. 60° 45′ 28″ N.	,0053726	$\frac{1}{305,07}$	39,01188
Portsoy . . 57 40 59	,0053732	$\frac{1}{305,13}$	,01188
Leith Fort . 55 58 41	,0053570	$\frac{1}{303,63}$	,01198
Clifton . . 53 27 43	,0053109	$\frac{1}{299,44}$	,01225
Arbury Hill . 52 12 55	,0053565	$\frac{1}{303,59}$	,01198
London . . 51 31 8	,0053151	$\frac{1}{299,81}$	,01223
Shanklin Farm 50 37 24	,0053163	$\frac{1}{299,92}$	,01222
	=,0053431	$\frac{1}{302,37}$	39,01206

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* See Remarks after the Experiment, in the Appendix.

Observations for the Latitude.

October 16, 1822. *By Stars.* Barometer 29,75. Thermometer 74°.

α Aquilæ.
Face of Instrument East.

	°	′	″
Readings 1st Vernier -	31	18	25
Readings 2d Vernier -		19	10
Observed merid. zen. dist. of α Aquilæ - -	31	18	47,5
Refraction -	+		33,5
*'s true merid. zenith dist.	31	19	21,0
*'s Declination - -	8	24	35,9 N
Latitude, face East -	22	54	45,1 S
Latitude, face West -	22	56	3,7
Latitude of Gloria Hill, Rio Janeiro - -	22	55	24,4 S

α Cygni.
Face of Instrument West.

	°	′	″
Readings 1st Vernier -	22	27	5
Readings 2d Vernier -		26	25
Observed meridian altitude of α Cygni - -	22	26	45
Refraction -	—	2	11,4
True altitude of α Cygni	22	24	33,6
True meridian zenith dist.	67	35	26,4
Declination of α Cygni -	44	39	22,7 N
Latitude, face West -	22	56	3,7 S

By the Sun.

November 3, 1822.
Barometer 29,76. Thermometer 92°.
Face of Instrument West.

	°	′	″
Readings 1st Vernier -	81	50	10
Readings 2d Vernier -		49	35
		99	45
⊙'s observed altitude, L. L.	81	49	52,5
⊙'s semi-diameter -	+	16	9,9
Apparent altitude ⊙'s centre	82	6	2,4
Refraction -	—		7,4
Parallax -	+		1,6
True altitude ⊙'s centre -	82	5	56,6
⊙'s meridian zenith distance	7	54	3,4
⊙'s Declination - -	15	1	55,8 S
South latitude, face West	22	55	59,2

November 8, 1822.
Barometer 29,82. Thermometer 85°.
Face of Instrument East.

	°	′	″
Readings 1st Vernier -	6	37	40
Readings 2d Vernier -		38	12
		75	52
Observed meridian zenith distance ⊙'s L. L. -	6	37	56
⊙'s semi-diameter -	—	16	11
Apparent meridian zenith distance ⊙'s centre -	6	21	45
Refraction -	+		6,0
Parallax -	—		1,5
Mer. zen. dist. ⊙'s centre true	6	21	49,5
⊙'s Declination - -	16	32	50,4 S
South latitude, face East	22	54	39,9
Ditto face West	22	55	59,2
Latitude by the Sun -	22	55	19,5 S

	°	′	″
Latitude by Stars -	22	55	24,4
Latitude by the Sun -	22	55	19,5
Mean latitude of Gloria Hill, Rio de Janeiro	22	55	22 South.

APPENDIX.

Second Series of Experiments at London, on the return from South America.

17th Aug. 1823, P.M. at Mr. Browne's. Clock gaining 0^{s},90. Barom. 29,86.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°						
66,2	12 18 33	1,17	°					
66,0	35 20	1,07	1,12	1007		86229,30	2,05	86231,35
	52 10	0,97	1,02	1010		29,81	1,70	31,51
	1 9 00	0,88	0,93	1010		29,81	1,41	31,22
	25 53	0,79	0,83	1013		30,32	1,12	31,44
66,9	42 47	0,72	0,76	1014		30,69	0,94	31,63
66,6	59 40	0,67	0,70	1013		30,32	0,80	31,12
66,4	Mean			1011,17	1009,17	86230,04	1,34	86231,38
18th. Aug. P. M. Clock gaining 0^{s},90.								Barom. 29,88.
67,2	12 13 25	1,15						
67,1	30 11	1,05	1,10	1006		86229,13	1,98	86231,11
	46 58	0,96	1,00	1007		29,30	1,64	30,94
	1 3 49	0,87	0,91	1011		29,98	1,35	31,33
	20 41	0,79	0,83	1012		30,15	1,12	31,27
67,1	37 34	0,72	0,76	1013		30,32	0,94	31,26
67,0	54 29	0,66	0,69	1015		30,66	0,78	31,44
67,1	Mean			1010,67	1008,67	86229,92	1,30	86231,22
19th Aug. P. M. Clock gaining 0^{s},60.								Barom. 29,80.
67,7	2 28 12	1,19						
67,9	44 59	1,08	1,13	1007		86229,00	2,09	86231,09
	3 1 47	0,98	1,03	1008		29,17	1,74	30,91
	18 37	0,90	0,94	1010		29,51	1,45	30,96
	35 31	0,82	0,86	1014		30,19	1,21	31,40
68,9	52 25	0,74	0,78	1014		30,19	0,99	31,18
68,5	4 19 17	0,69	0,71	1012		29,85	0,82	30,67
68,3	Mean			1010,83	1008,83	86229,65	1,38	86231,03

20th Aug P. M. at Mr. BROWNE's. Clock gaining $0^s,60$. } Barom. 29,83.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
67,4	12 1 39	1,13						
67,1	18 27	1,03	1,08	1008		86229,17	1,91	86231,08
	35 18	0,94	0,98	1011		29,68	1,57	31,25
	52 12	0,84	0,88	1014		30,19	1,27	31,46
	1 9 7	0,77	0,80	1015		30,36	1,05	31,41
67,9	26 1	0,71	0,74	1014		30,19	0,89	31,08
67,7	42 56	0,65	0,68	1015		30,36	0,76	31,12
67,5	Mean			1012,83	1010,83	86229,99	1,24	86231,23

21st. Aug. P. M. Clock gaining $1^s,00$. Barom. 29,84.

66,2	11 30 31	1,25						
66,0	47 17	1,13	1,19	1006		86229,23	2,32	86231,55
	12 4 5	1,02	1,08	1008		29,57	1,91	31,48
66,3	20 54	0,93	0,97	1009		29,74	1,54	31,28
65,9	37 45	0,83	0,88	1011		30,08	1,27	31,35
66,3	54 39	0,75	0,79	1014		30,59	1,02	31,61
66,0	1 11 33	0,69	0,72	1014		30,59	0,85	31,44
66,1	Mean			1010,33	1008,33	86229,97	1,48	86231,45

21st Aug. P. M. Clock gaining $1^s,00$. Observed by Captain KATER. } Barom. 29,85.

66,4	2 59 5	1,29						
66,1	3 15 49	1,16	1,22	1004		86228,89	2,43	86231,32
	32 36	1,05	1,10	1007		29,40	1,98	31,38
	49 25	0,95	1,00	1009		29,74	1,64	31,38
	11 16	0,85	0,90	1011		30,08	1,32	31,40
66,7	23 7	0,76	0,80	1011		30,08	1,05	31,13
66,4	40 0	0,71	0,73	1013		30,42	0,87	31,29
66,4	Mean			1009,17	1007,17	86229,77	1,55	86231,32

22d Aug. P. M. at Mr. Browne's. Clock gaining $0^s,80$. Bar. 29,72.

Temp. Fahrenheit.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h, m. s.	°	°					
66,5	11 47 7	1,20						
66,2	12 3 53	1,09	1,14	1006		86229,03	2,13	86231,16
	20 42	0,99	1,04	1009		29,54	1,77	31,31
66,5	37 32	0,88	0,94	1010		29,71	1,44	31,15
66,3	54 24	0,80	0,84	1012		30,05	1,15	31,20
66,7	1 11 19	0,73	0,76	1015		30,56	0,94	31,50
66,4	28 14	0,67	0,70	1015		30,56	0,80	31,36
66,4	Mean			1011,17	1009,17	86229,91	1,37	86231,28

RESULTS.

Date.		Barometer.	Thermom.	Vibrations in 24 hours.	Correction for Temp.	Vibrations in 24 hours at Temp. 68 degrees.
		Inches.	°			
August	17	29,86	66,4	86231,38	— 0,68	86230,70
——	18	29,88	67,1	86231,22	— 0,38	86230,84
——	19	29,80	68,3	86231,03	+ 0,11	86231,14
——	20	29,83	67,5	86231,23	— 0,21	86231,02
——	21	29,84	66,1	86231,45	— 0,80	86230,65
Observed by Capt. Kater	21	29,85	66,4	86231,32	— 0,68	86230,64
——	22	29,72	66,4	86231,28	— 0,66	86230,62
Mean		29,83	66,9			86230,80
Correction for Buoyancy . .						+ 5,93
Ditto for Elevation						+ ,22
No. of vibrations of the pendulum in London in Aug. 1823						86236,95
Ditto in May 1820, before the experiments in South America were made						86235,98
Difference between the Experiments of 1820 and 1823 .						0,97

REMARKS.

As it was not possible that so great a difference could arise from errors of observation, it became an object of anxious inquiry to discover the cause. Captain Kater was disposed to assign it to an accident which had happened to the pendulum at San Blas, but which I, at first, imagined inadequate to such an effect. The accident was this: the pendulum, when not in use, was, as usual,

raised by means of a screw, so that the knife edge was lifted clear of the agate planes on which it vibrated during the experiments. This screw being too small, or having some flaw in it, unexpectedly broke at San Blas before the experiments there were begun; and although the knife edge was not raised more than the twentieth of an inch, yet, as the pendulum weighed more than 15 lbs., the fall might, he thought, have altered the form of so delicate an edge in a slight degree, and thus have virtually lessened the distance between the point of suspension and the centre of oscillation.*

As the whole pendulum had acquired a coating of oxide, with the exception of the tail piece, which was lackered, I was desirous of ascertaining in what manner and to what degree its vibrations would be affected by this partial addition of weight; and for this purpose the following experiments were made. The vibrations of the pendulum in its oxydized state having been determined, 10 grains of weight were affixed at $\frac{2}{3}$ of the length of the bar (measured through the ball), from the point of support, that being supposed to be near the centre of oscillation of the oxide. This had for its object to discover, before cleaning the pendulum, what would be the effect of an addition of weight at that place. On swinging it accordingly, the number of vibrations was increased 0,83 in 24 hours. It was then taken to the Mint, and the weight, carefully determined by Mr. BARTON in one of his delicate balances, was found to be 15 lb. 10 oz. 14 dwt. $12\frac{1}{2}$grs. It was next cleaned by Captain KATER, by means of diluted sulphuric acid, and afterwards washed with a solution of soda in water, and being effectually dried, was again weighed, when it was found to have lost exactly $24\frac{3}{4}$ grains. Coincidences were now taken on three succeeding days, and the number of vibrations of the pendulum in its clean state proved to be fewer than when it was coated with oxide by only 0,73 of a vibration. Since no more than $\frac{1}{5}$ part of the oxide removed could be oxygen, only $\frac{1}{5}$ of the above difference between its vibrations when clean and when coated, or 0,14, can be ascribed to additional weight since it was formerly swung in 1820; the real difference, however, to be accounted for being 0,97, this cause is manifestly inadequate to the effect. I have therefore thought it right, after attentively considering every other possible manner in which the pendulum could have been altered, to adopt the idea which had been suggested, and which was eventually proved to be correct, since the knife edge, upon removal after the experiments were over, was found to be distinctly rounded. To obtain the most correct results, I have accordingly used the vibrations made in London in 1820, to compare with the experiments made before the accident, and the vibrations recently determined in London for comparing with those made after it; an arrangement rendering the resulting ellipticities entirely independent of that circumstance.

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* If the knife edge be supposed to have become cylindrical, the virtual point of suspension, it has been demonstrated, would be at the distance of the radius of curvature of this cylindrical portion below its surface, and the number of vibrations of course be greater than before.

XXI. *Second Part of the paper on the Nerves of the Orbit. By* CHARLES BELL, *Esq. Communicated by Sir* HUMPHRY DAVY, *Bart. Pres. R. S.*

Read June 19, 1823.

IN these papers I endeavour, to the utmost of my power, to distinguish between the facts which I am able to substantiate, and the hypothesis by which I have been directed in my inquiries. I hope that the importance of the facts may give some bias in favour of that mode of reasoning by which they have been discovered, and an additional interest to anatomical studies.

In my endeavour to arrange the nerves of the orbit, I encounter, in the first step, all the difficulties of my subject; for although there be only nine nerves properly enumerated as proceeding from the brain, six of these go to the eye; the second, third, fourth, part of the fifth, sixth, and seventh, go into the orbit, and may be said to be concentrated into a space no larger than a nut-shell.

In this investigation it is not always possible to give demonstrative evidence, or to answer opposition by cutting across a nerve; here we must proceed on a minute investigation of the anatomy, and by reasoning, rather than by experiment: yet I shall demonstrate what was stated hypothetically, in a former paper, that there is a correspondence between the compound functions of an organ, and the nerves transmitted to it.

Of the function of the ophthalmicus, a division of the fifth nerve.

We are, in the first place, to inquire by what nerve the common endowment of sensibility is bestowed upon the membranes and surfaces of the eye. On recurring to this subject we are reminded, that the sensibilities of the body differ as much in kind as in degree; that the sensation of pain is provided to rouse our activity, and guard us against violence, or, by means more direct, to excite instinctive motions, which shall anticipate the most rapid actions of the will, and serve as a more perfect safeguard. The trigeminus, or fifth nerve, bestows upon all the surfaces of the head and face, external and internal, that sensibility which is enjoyed by the rest of the body through the spinal nerves. But through some of its branches is also bestowed that distinct sense on certain parts, for the purpose of drawing the muscles into combination; as for example, that fine sensibility of the surface of the eye to the presence of minute particles, which at once excites the flow of tears, and draws the muscles into a combination to expel the offensive matter.

It has been shown in a preceding paper, by experiment, that on dividing the branch of the fifth nerve to the cheek and lips, the skin was deprived of sensibility, although in possession of other nerves, and enjoying muscular activity. The same has been proved in regard to this ophthalmic division; for if that branch of it which comes through the orbit and mounts upon the forehead, be divided, the skin will be deprived of sensibility.

These facts are so strong, that when supported by the symptoms of disease they afford no apology for deep dissec-

tion in the living animal, and authorize the conclusion, that all the branches of the same division resemble each other in function, and bestow sensibility on the parts within, as well as on those without.

That the ophthalmic nerve may be deprived of its function, and the parts supplied by it of their sensibility, we may learn from the following instance, communicated to me by Mr. CRAMPTON, of Dublin. To understand the inference from the following short narrative, it is only necessary to remember, that the nerve in question goes through the orbit, supplying the parts contained in it, but that it also extends its branches to the angle of the eye, eyelids, and forehead. "A few days after the discharge from the ear had ceased, the eye became entirely insensible to the touch. This loss of feeling extended to the lining of the eyelids, to the skin covering them, and to the skin of the cheek and forehead, for about an inch surrounding the eye: it did not go beyond the middle line of the face. When she told me her eye was *dead* (as she expressed it), to be certain, I drew my finger over its surface; and so far was this from giving her pain, that she assured me she could not feel that I was touching it at all. The eyelids made no effort to close while I was doing this, but the conjunctiva appeared sensible to the stimulus, as a number of vessels on the surface of the eye became immediately injected with blood."

Here we have an insensibility of the eye itself corresponding with the insensibility of the skin, which latter part we know possesses sensibility through the *fifth nerve;* and we therefore conclude, that it is the affection of the same nerve near its root, to which we have to attribute the insensibility

of the surfaces of the eye, as well as of the skin around the eye.

By experiment it can farther be made evident, that the sensibility of the eye enjoyed through the ophthalmic nerve, does not bestow on the organ directly, the power of combining the muscles, either for the defence of the eye, or for any other purpose. The impression must be referred back to the brain, and the muscles excited by their proper nerves. I have not been able to excite the motion of the eye by irritating the ophthalmic division of the fifth after the division of its root,* and in the instance just given, the eyelids did not move when the surface of the eye was irritated, because no sensation was conveyed inward to the sensorium, and consequently no mandate transmitted from it. The young lady could see, and could move the eye and eyelids; the eye itself was irritated by touch, as appeared from the rising inflammation; but by the insensibility of the ophthalmic nerve, a link was lost in the relation necessary to join the action of the muscles to the sensibility of the surface.

Of the nerves performing the involuntary motions.

We have just seen that nerves in great profusion come out upon the eyelids and forehead, and until these experiments were made, it was supposed that they directed the motions of the forehead and eyelids. But I have found that they have nothing to do with this function. On the contrary, a very small branch of the respiratory nerve of the face, that nerve which comes out before the ear, controuls the motions of the

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* In attempting to excite the muscles of the eye by galvanism sent through the fifth nerve, the muscles of the jaw were affected.

forehead and eyelids. If this small nerve be divided, then the motions of the eyelids are lost, and they remain open. The inquiries instituted in the first part of this paper, give a lively idea of the consequences of this imperfection; showing that the eye being unguarded and unwashed, becomes dry by evaporation and inflames, and the cornea becomes opaque. It is unnecessary to point out the importance of this fact to the operating surgeon.

It has been asked, why should this nerve be called respiratory; and what have the actions of respiration to do with the eye-lids? The name was given to excite attention to certain relations; that the question might be asked, and the connections of remote parts noticed and remembered. These connections of remote parts are so curious, the knowledge of them is sometimes so useful, and they are so immediately related to the present subject, that I may be permitted to explain them.

During the state of excitement of the respiratory organs, a very extensive consent of the muscular frame is necessary to bind together and support the textures, that they may bear the strain either during violent efforts of the body, or in coughing, sneezing, &c. We may take the act of sneezing, as a familiar example of the manner in which the eye is guarded during a sudden and violent act of expiration.

At the instant of this convulsive action of the respiratory muscles, a violent impulse is communicated to the head along the column of blood in the vessels of the head and neck. Every body is sensible of the eye flashing light, but the cause is mistaken; for it is supposed to be the impulse of blood forced into the eye; whereas it is the contraction of

the eyelids to counteract the force of the impulse, and to guard the delicate texture of the eye. If the eyelids be held open during the act of sneezing, no sensation of light will be experienced, because the contraction of the eyelids upon the eyeball is prevented.

Can we believe this action of the muscle of the eyelids in combination with the action of the respiratory muscles, to be an accidental connection? Is it not rather a provision to compress and support the vascular system of the eye, and to guard it against the violent rush of blood which attends certain acts of respiration? If we open the eyelids of a child to examine the eye while it is crying, and struggling with passion, by taking off the natural support from the eye, the blood at the same time being forced violently into the head by the act of respiration, we shall see the conjunctiva suddenly fill with blood, and the eyelid everted.

The respiratory nerve of the face performs two offices, one of which is voluntary, as in moving the cheeks and lips in speech; and the other involuntary, as in moving the nostrils in breathing during sleep or insensibility. In like manner that branch of the respiratory nerve which is prolonged to the eyelids performs a double office, contracting the eyelids by volition, and also producing those involuntary winking motions of the eyelids which disperse the tears, and preserve the lucid surface clear.*

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* Having distinguished the functions of the fifth and seventh nerves, a question still remains, whether the different operations performed by any one of them, depend on the exercise of distinct filaments? I believe these filaments to be distinct nerves bound up together, and analogy would lead me to suppose them capable of distinct functions; but I cannot demonstrate this unless in the spinal nerves, where the roots are separate.

But it has been observed, in the First Part of this Paper, that the shutting of the eyelids is not the only part of this act of preservation, and that the motions of the eyelids are attended with a rolling of the eyeball. How is this relation between the eyelids and eyeball established? This leads to an examination of the fourth nerve.

The fourth nerve.

This is a fine nerve, which takes its origin from the brain, at a part remote from all the other nerves which run into the orbit. It threads the intricacies of the other nerves without touching them, and is entirely given to one muscle, the superior oblique. We may observe too, that this singularity prevails in all animals. What office can this nerve have in reference to this one muscle? Why is it's root, or source, different from the other nerves, from the nerve of vision, the nerve of common sensibility, and the nerve of voluntary motion? We now reflect, with increased interest, on the offices of the oblique muscles of the eye, observing that they perform an insensible rolling of the eyeball, and hold it in a state of suspension between them. We have seen that the effect of dividing the superior oblique was to cause the eye to roll more forcibly upwards; and if we suppose that the influence of the fourth nerve is, on certain occasions, to cause a relaxation of the muscle to which it goes, the eyeball must be then rolled upwards.*

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* The nerves have been considered so generally as instruments for stimulating the muscles, without thought of their acting in the opposite capacity, that some additional illustration may be necessary here. Through the nerves is established the connection between the muscles, not only that connection by which muscles

The course of inquiry leads us, in the next place, to observe the vicinity of the root of this fourth nerve, to the origin of the respiratory of the face, and we find them arising from the same track of fibrous substance. The column of medullary matter which constitutes that part of the medulla oblongata from which the respiratory nerves arise, terminates upwards, or at its anterior extremity, just under the corpora quadrigemina, and there the fourth arises. Is it possible then, we say, that there can be any correspondence between the general act of respiration, and the rolling of the eye? Led thus to make the experiment, I was gratified to find it so easy to give the proof. On stopping the nostrils with the handkerchief, every effort to blow the nose will be attended by a rapid rising of the cornea under the upper eyelid. And on every occasion when the eyelids suffer contraction through the agency of the respiratory nerve of the face, as in sneezing, the eyeball is rolled upwards through the agency of the fourth nerve.

I might, perhaps, be satisfied with having made the observation of these two facts; first, that there is such a combi-

combine to one effort, but also that relation between the classes of muscles by which the one relaxes while the other contracts. I appended a weight to a tendon of an extensor muscle, which gently stretched it and drew out the muscle; and I found that the contraction of the opponent flexor was attended with a descent of the weight, which indicated the relaxation of the extensor. To establish this connection between two classes of muscles, whether they be grouped near together, as in the limbs, or scattered widely as the muscles of respiration, there must be particular and appropriate nerves to form this double bond, to cause them to conspire in relaxation as well as to combine in contraction. If such a relationship be established, through the distribution of nerves, between the muscles of the eyelids and the superior oblique muscle of the eyeball, the one will relax while the other contracts.

nation of the motions of the eyeball and eyelids as I have before noticed; and secondly, that the nerves which move the eyelids, and the nerve of the obliquus muscle of the eyeball, are associated at their roots; but I should not do full justice to this interesting subject, if I did not attempt something farther.

It is plain that we must consider the nerves and muscles of the eyelids in a double capacity, in their voluntary, and involuntary actions. In the first, the motions of the eyelids combine with the whole muscles of the eyeball, as we may perceive in the voluntary contractions and squeezing of the eye; but in the insensible and involuntary motions of the eyelids, there would be no sympathy with the muscles of the eyeball, and therefore no correspondence in the motion of these parts, without a nerve of the nature of the fourth; that is, a nerve which having diverged from the root of the respiratory nerves, takes its course to the oblique muscles. In one word, the connection of its root declares the office of this nerve.

The expression of the eye in passion, confirms the truth of this relation being established by a respiratory nerve, and consequently by a nerve of expression. In bodily pain, in agony of mind, and in all this class of passions, the eyes are raised and dragged, in conjunction with the changes to which the other features are subjected. If it be asked now, as it has been asked for some hundred years past, why the fourth nerve goes into the orbit, where there are so many nerves, why it is so distant in its origin from the other nerves, and why it sends off no twig or branch, but goes entirely to one muscle of the eye? The answer is, to provide for the insen-

sible and instinctive rolling of the eyeball; and to associate this motion of the eyeball with the winking motions of the eyelids; to establish a relation between the eye and the extended respiratory system: all tending to the security or preservation of the organ itself.

Of the voluntary nerves.

The voluntary nerves of the eye are the Third and Sixth. The third nerve arises from the crus cerebri, that track of medullary matter which gives off all the nerves purely of volition. It is given to the muscles of the eye generally, and to no part but muscles. For these reasons we retain the name *motor oculi*, given by Willis, although his reasons for calling it so were fanciful and unsatisfactory. The Fifth nerve, by its ophthalmic division, gives branches to the muscles of the eye, but not so profusely as to the surrounding parts; and not more than sufficient to give them sensibility in the degree possessed by muscular substance generally. Since the branches of this fifth nerve, transmitted to the muscles of the eyelids and forehead, do not minister in any degree to muscular action there, it would be unwarrantable to suppose that they served the purpose of giving action to the muscles within the orbit. For these reasons, I conceive the Third nerve to be that which gives volition to the muscles of the eye, and that it is, of all the nerves of the body, the most perfectly and directly under the power of the will.

The *sixth nerve* is called *abducens*, and *motor externus*. With regard to its origin and distribution, there is no obscurity in this nerve; it arises from the same track of medullary matter which gives rise to the motor nerves, and it is distributed to

a voluntary muscle, the *rectus externus.* In this respect it is like a subdivision of the Third, and without doubt it is a voluntary nerve; but there is a circumstance in its connection which I cannot explain. It receives a gross branch from the great visceral nerve called Sympathetic. This nerve, ascending through the base of the skull, unites with the Sixth nerve as it is entering the orbit. Some having proceeded so far, would be inclined to call this an accidental connection, and so leave it; but similar investigations for many years have brought me to the conviction that there is no accident in an animal body; and Comparative Anatomy proves this to be a regular established relation.

To return to the consideration of these nerves of volition as they regard the eye, we may affirm, that although they want sensibility in the common acceptation of the term, they no doubt furnish the mind with the rudiments of certain sensations, and so far resemble the nerves of the senses. From experiments narrated in the first part of this paper, it appears, that we are sensible to the degree of agency exercised by the voluntary muscles of the eye. These nerves, the Third and Sixth, although they receive no external impression, are nevertheless agents which give rise to the perceptions of place or relation, in aid of that sensibility enjoyed by the optic nerve and retina.

I hope I have now unravelled the intricacy of the nerves of the head, and have correctly assigned to each nerve its proper office. In our books of Anatomy, the nerves are numbered according to the method of Willis, an arrange-

ment which was made in ignorance of the distinct functions of the nerves, and merely in correspondence with the order of succession in which they appear on dissection.

The first nerve is provided with a sensibility to effluvia, and is properly called olfactory nerve.

The second is the optic nerve, and all impressions upon it excite only sensations of light.

The third nerve goes to the muscles of the eye solely, and is a voluntary nerve by which the eye is directed to objects.

The fourth nerve performs the insensible traversing motions of the eyeball. It combines the motions of the eyeball and eyelids, and connects the eye with the respiratory system.

The fifth is the universal nerve of sensation to the head and face, to the skin, to the surfaces of the eye, the cavities of the nose, the mouth and tongue.*

The sixth nerve is a muscular and voluntary nerve of the eye.

The seventh is the auditory nerve, and the division of it, called *portio dura*, is the motor nerve of the face and eyelids, and the respiratory nerve, and that on which the expression of the face depends.

* In this view of the fifth nerve, I have not touched upon its resemblance to the spinal nerves. But if we had ascended from the consideration of the spinal nerves to the nerves of the head, we should then have seen that the fifth was the spinal nerve of the head; that it had a ganglion at its root, a double origin, and from its power over the muscles of the jaws and mastication, that it was a double nerve in function, being that nerve which bestows sensibility, at the same time that it sends branches to the original muscles; that is to say, to that class of muscles which are common to animals in every gradation. In all these respects it resembles the spinal nerves.

The eighth, and the Accessory nerve, are respiratory nerves.

The ninth nerve is the motor of the tongue.

The tenth is the first of the spinal nerves ; it has a double root and a double office ; it is both a muscular and a sensitive nerve.

Had I taken the nerves of any other complex organ rather than of the eye, I should have had an easier task. If I had taken the nerves of the tongue, I should have been able to prove by experiment, and in a manner the most direct, that the three nerves belong to three distinct functions, and stand related to three different classes of parts. I could have shown that taste and sensibility belong to the office of the fifth nerve, voluntary motion to the ninth, and deglutition to the glosso-pharyngeal nerve of the tongue.

In concluding these papers, I hope I may be permitted to offer a few words in favour of anatomy, as a means better adapted for discovery than experiment. The question lies between observation and experiment, and it may be illustrated by astronomy and chemisty. In the first, the objects being beyond the influence of man, he makes observations, not experiments ; and the science at length attains a state of perfection which raises our estimate of the human intellect. In the latter, for the most part, the subjects lie out of the sphere of mutual influence ; they must be brought together by artifice, and chemistry becomes a science of experiment. But anatomy is more allied to the former than to the latter

science, in as much as things are obvious to the eyes. In the animal body the parts present distinct textures, and are laid in a natural and perfect order; it is necessary only to trace the tubes, or to observe the symmetrical order of the nervous cords, in order to discover their respective uses: the motions, whether of the solid or fluid parts, are so regular and uniform, that the whole offers a subject for observation and induction. Anatomy is already looked upon with prejudice by the thoughtless and ignorant: let not its professors unnecessarily incur the censures of the humane. Experiments have never been the means of discovery; and a survey of what has been attempted of late years in physiology, will prove that the opening of living animals has done more to perpetuate error, than to confirm the just views taken from the study of anatomy and natural motions.

In a foreign review of my former papers, the results have been considered as a farther proof in favour of experiments. They are, on the contrary, deductions from anatomy; and I have had recourse to experiments, not to form my own opinions, but to impress them upon others. It must be my apology, that my utmost efforts of persuasion were lost, while I urged my statement on the grounds of anatomy. I have made few experiments; they have been simple, and easily performed; and I hope are decisive.

If we turn to the opinions which have been entertained on the subject of the brain and nerves, we find one theory to have prevailed from the Greek authors to the time of Willis, and to have descended from him with little alteration, to modern writers. The brain has been supposed to secrete and supply a nervous fluid, and the nerves to be the conduit

pipes for its conveyance. In every age the brain has been considered a common sensorium, and all the nerves to be capable of conveying sensation, unless when they had ganglia. If ganglia intervened, then the nerves were said to be cut off from the brain; and those so distinguished were called vital nerves, neither serving the purpose of governing the muscles, nor of conveying sensation. With all this apparent simplicity of doctrine, there never has been presented such a crude heap of errors, in the history of any department of science.

These notions were obviously founded on the mistake, that the same nerve served different purposes, and that a fluid moved in the same tube outwards to stimulate the muscles, and inwards to convey sensation of external impressions. So inconsistent are those opinions with the structure of the frame, that the simplest dissection proves them false.

So far is it from being true that ganglia cut off sensation, that I have ascertained, and proved by experiment, that all the nerves, without a single exception, which bestow sensibility from the top of the head to the toe, have ganglia on their roots; and those which have no ganglia are not nerves of sensation, but are for the purpose of ordering the muscular frame.

The hypothesis, that the nervous fluid streams out from the great officina along the nerves, has had an unfortunate influence in directing the labours of the experimentalists. During the last age it kept the pupils of HALLER engaged in inquiries regarding the influence of the nerves: *de nutritione imprimis nervosa:* and *de nervorum in arterias imperio;* and the interest of this question has not subsided, but, on the contrary, has encreased among us.

This notion of a fluid moving backwards and forwards in the tubes of the nerves, equally adapted to produce motion and sensation, has perpetuated the error, that the different nerves of sensation are appropriated to their offices by the texture of their extremities, "that there exists a certain relation between the softness of the nervous extremities, and the nature of the bodies which produce an impression on them." On the contrary, every nerve of sense is limited in its exercise, and can minister to certain perceptions only. Whatever may be the nature of the impulse communicated to a nerve, pressure, vibration, heat, electricity, the perception excited in the mind will have reference to the organ exercised, not to the impression made upon it. Fire will not give the sensation of heat to any nerve but that appropriated to the surface. However delicate the retina be, it does not feel like the skin. The point which pricks the skin being thrust against the retina, will cause a spark of fire or a flash of light. The tongue enjoys two senses, touch and taste; but by selecting the extremity of a particular nerve, or what is the same thing, a particular papilla, we can exercise either the one or the other sense separately. If we press a needle against a nerve of touch, we shall feel the sharpness, and know the part of the tongue in contact with the point; but if we touch a nerve of taste, we shall have no perception of form or of place, we shall experience a metallic taste.

The innovations of the celebrated continental authority Bichat, did not bring us a step nearer the truth. When he at once threw off respect for his contemporaries, and for the authority of those who had preceded him, he equally disregarded the facts of anatomy. There may be merit in taking

new views of a subject, but Bichat was continually holding a thing up by the wrong end, and presenting it in an aspect so singular, as to puzzle any one to say whether or not it was that with which he had been long familiar ; accordingly, what had been termed the sympathetic system of nerves, he called the ganglionic system ; although they are not more distinguishable by ganglia than the other nerves, upon which indeed the ganglia are remarkable for their size, number, and regularity. These ganglia must not be thrown out of the system altogether, merely because they are contained within the skull and vertebræ, which circumstance should rather mark their importance.

Bichat persuaded himself that his ganglionic system was isolated, and a thing by itself ; when, on the contrary, the connections of this part of the nervous system are universal. The wide spreading fifth pair, and the thirty spinal nerves, give large and conspicuous roots to this system. It exhibits a tissue extending universally.

It was a still more unfortunate mistake of this ingenious physiologist, to suppose the sympathetic nerve to be the same with that, which in the lower animals (the vermes), is seen coursing from one extremity of the body to the other. In the leech, or worm, those nerves produce union and concatenation of all the voluntary motions, and bestow sensibility as well as motion ; yet he saw in the sympathetic system of the human body, only the developement of the same system of nerves, although he was aware that in man the sympathetic nerve bestowed neither sensibility nor the power of motion.

Bichat announced his system with a popular eloquence, which had a very remarkable influence over all Europe.

Physiologists yielding to him, mistook the importance of the several parts of the nervous system; and even the multiplied experiments of Le Gallois failed to convince them of the nature of the spinal marrow.

The experiments of M. Le Gallois were of the rudest kind possible. The spinal marrow was cut across, or destroyed, by passing skewers into the spinal canal, and the effects were observed; as if the spinal marrow were a simple body. Whereas, by such destruction of its substance, the original ganglia, which form a series along the spine, must have been hurt; the track of nervous matter which gives rise to the nerves of sensation: that also which gives roots to the nerves of voluntary motion: and the lateral column connected with the offices of respiration, must all have been destroyed by such coarse experiments. It cannot surprise us that the results were obscure and contradictory.

But the most extravagant departure from all the legitimate modes of reasoning, although still under the colour of anatomical investigation, is the system of Dr. Gall. It is sufficient to say, that without comprehending the grand divisions of the nervous system, without a notion of the distinct properties of the individual nerves, or having made any distinction of the columns of the spinal marrow, without even having ascertained the difference of cerebrum and cerebellum, Gall proceeded to describe the brain as composed of many particular and independent organs, and to assign to each the residence of some special faculty.

When the popularity of these doctrines is considered, it may easily be conceived how difficult it has been, during their successive importations, to keep my Pupils to the examples

of our own great Countrymen. Surely it is time that the schools of this kingdom should be distinguished from those of France. Let physiologists of that country borrow from us, and follow up our opinions by experiments,* but let us continue to build that structure which has been commenced in the labours of the MONROS and HUNTERS.

The whole history of medical literature proves, that no solid or permanent advantage is to be gained, either to medical or general science, by physiological experiments unconnected with anatomy. To disregard the anatomy of the nervous system, or to take it in the gross; to make a new science *of life*; and influenced by a false analogy to call it a fluid; to attempt to direct it along a cord or a wire, is to transgress all the rules of philosophical enquiry, and must be attended with the rapid decline of anatomical studies. They will be considered as imposing restraints on genius, or be rejected as useless; and with them pathology, and all that is most necessary to medical science, will fall into disuse.

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* See the experiments of M. MAGENDIE on the distinctions in the roots of the spinal nerves.

XXII. *An Account of Experiments made with an Invariable Pendulum at New South Wales, by Major-General Sir* Thomas Brisbane, *K. C. B. F. R. S. Communicated by Captain* Henry Kater, *F. R. S. in a Letter to Sir* Humphry Davy, *Bart. Prés. R. S.*

Read June 19, 1823.

Sir,

In compliance with the request of Major General Sir Thomas Brisbane, I have the honour to lay before the Royal Society the results which I have deduced from his experiments, made with an invariable pendulum at London and at Paramatta.

The pendulum employed belongs to the Board of Longitude, and is of the same construction as that which I used at certain stations of the Trigonometrical Survey of Great Britain. Before Sir Thomas Brisbane's departure, I observed the following series of coincidences at Mr. Browne's house in Portland Place, London, in order to determine the number of vibrations made by this pendulum in twenty-four hours.

April 18th, 1821, London.

Clock gaining $0,^s83$ Barometer, 29,70 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°				°	
56,3	10 36 6	1,30	1,26	542			2,61	
	45 8	1,23	1,20	543			2,36	
	54 11	1,17	1,14	543			2,13	
	11 3 14	1,12	1,09	543			1,95	
	12 17	1,06	1,03	543			1,74	
	21 20	1,01	0,98	543			1,58	
	30 23	0,96	0,93	545			1,42	
	39 28	0,91	0,89	544			1,30	
	48 32	0,87	0,85	544			1,18	
	57 36	0,83	0,80	544			1,05	
58,6	12 6 40	0,78						
57,5	Mean			543,4	541,4	86082,83	1,73	86084,56

April 19. Barometer 29,60 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
56,4	11 22 30	1,34	1,30	542			2,78	
	31 22	1,27	1,23	543			2,48	
	40 35	1,20	1,16	543			2,21	
	49 38	1,13	1,09	543			1,95	
	58 41	1,06	1,03	543			1,74	
	12 7 44	1,01	0,97	544			1,54	
	16 48	0,94	0,92	544			1,38	
	25 52	0,90	0,88	544			1,27	
	34 56	0,86	0,84	544			1,15	
	44 0	0,82	0,79	544			1,02	
58,2	53 4	0,77						
57,3	Mean			543,4	541,4	86082,83	1,75	86084,58

April 20. Barometer 29,58 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
57,7	10 40 22	1,24	1,21	542			2,40	
	49 24	1,18	1,15	544			2,17	
	58 28	1,12	1,09	544			1,95	
	11 7 32	1,06	1,03	542			1,74	
	16 34	1,00	0,97	545			1,54	
	25 39	0,94	0,92	543			1,38	
	34 42	0,90	0,88	545			1,27	
	43 47	0,87	0,85	545			1,18	
	52 52	0,83	0,80	542			1,05	
	12 1 54	0,78	0,75	542			0,92	
59,2	10 56	0,73						
58,4	Mean			543,4	541,4	86082,83	1,56	86084,39

April 21. Barometer 29,80 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Corrections for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°				°	
58,8	10 24 41	1,33	1,28	541			2,69	
	33 42	1,24	1,21	542			2,40	
	42 44	1,18	1,15	542			2,17	
	51 46	1,13	1,10	542			1,99	
	11 0 48	1,07	1,04	542			1,77	
	9 50	1,02	0,99	542			1,60	
	18 52	0,97	0,94	543			1,45	
	27 54	0,92	0,89	544			1,30	
	36 58	0,87	0,85	542			1,18	
	46 0	0,83	0,81	542			1,08	
60,6	55 2	0,80						
59,7	Mean			542,2	540,2	86082,13	1,76	86083,89

April 22. Barometer 29,98 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Corrections for Arc.	Vibrations in 24 hours.
58,8	10 25 59	1,29	1,26	542			2,61	
	31 1	1,23	1,19	542			2,33	
	44 3	1,16	1,13	543			2,09	
	—	1,10	1,03	544			1,74	
59,5	11 2 10	1,03						
59,1	Mean			542,75	540,75	86082,44	2,19	86084,63

April 23. Barometer 29,53 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Corrections for Arc.	Vibrations in 24 hours.
59,6	10 19 8	1,31	1,27	540			2,65	
	28 8	1,23	1,20	542			2,36	
	37 10	1,17	1,14	540			2,14	
	46 10	1,11	1,08	542			1,91	
	55 12	1,05	1,02	542			1,71	
	11 4 14	1,00	0,97	542			1,54	
	13 16	0,95	0,93	540			1,42	
	22 16	0,91	0,89	543			1,30	
	31 19	0,87	0,84	542			1,15	
	40 21	0,82	0,80	542			1,05	
62,0	49 23	0,78						
60,8	Mean			541,5	539,5	86081,61	1,72	86083,33

From the preceding observations we have the following results.

Vibrations of the Pendulum at London.					
Date.	Barom.	Therm.	Vibrations in 24 hours.	Corr. for Temp.	Vibrations in 24 hours, at 60 degrees.
	Inches.	°			
April 18	29,70	57,5	86084,56	— 1,06	86083,50
19	29,60	57,3	86084,58	— 1,14	86083,44
20	29,58	58,4	86084,39	— 0,68	86083,71
21	29,80	59,7	86083,89	— 0,13	86083,76
22	29,98	59,1	86084,63	— 0,38	86084,25
23	29,53	60,8	86083,33	+ 0,34	86083,67
	29,70	58,8			86083,72

The mean height of the barometer being 29,70 inches, and that of the thermometer 58°,80, if the specific gravity of the pendulum be taken at 8, the correction for the buoyancy of the atmosphere will be 6,45. Adding this to the mean number of vibrations before found, we obtain 86090,17 for the number of vibrations which would be made in a mean solar day at the temperature of 60°, and in a vacuum. The corrections applied for temperature are calculated on the supposition of the expansion of this pendulum being the same as that of the one before alluded to, which, as both are constructed of plate brass, will probably not occasion any sensible error.

On the completion of the preceding observations, the following were made by Sir THOMAS BRISBANE, and Mr. RUMKER.

April 25th, 1821, by Sir THOMAS BRISBANE, London.

Clock, mean time. Barometer 29,62 Inches.

Temp.	Time of co-incidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Obs. Vibrations in 24 hours.	Corr. for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°				°	
63,0	10 51 14	1,55						
	11 0 12	1,48	1,51	538			3,74	
	9 12	1,40	1,44	540			3,39	
	18 10	1,32	1,36	538			3,03	
	27 10	1,24	1,28	540			2,69	
	36 8	1,19	1,21	538			2,39	
	45 8	1,12	1,15	540			2,16	
	54 9	1,08	1,10	541			1,98	
	12 3 9	1,02	1,05	540			1,80	
	12 9	0,98	1,00	540			1,64	
64,4	21 10	0,92	0,95	541			1,48	
63,7	Mean			539,6	537,6	86079,76	2,43	86082,19

April 26, by Mr. RUMKER.

Clock losing $0^{s},3$. Barometer 29,70 Inches.

Temp.	Time of co-incidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Obs. Vibrations in 24 hours.	Corr. for Arc.	Vibrations in 24 hours.
64,5	11 16 21	1,29						
	25 20,5	1,25	1,27	539,5			2,64	
	34 19	1,20	1,22	538,5			2,44	
	52 20	1,08	1,14	541			2,13	
	12 1 20	1,04	1,06	540			1,84	
	10 20	0,97	1,00	540			1,64	
	19 19	0 93	0,95	539			1,48	
	28 22	0,88	0,90	543			1,33	
	37 22	0,83	0,85	540			1,18	
	46 22,5	0,78	0,80	540,5			1,05	
65,7	55 25	0,73	0,75	542,5			0,92	
65,1	Mean			540,4	538,4	86079,94	1,67	86081,61

April 27, by Sir THOMAS BRISBANE.

Clock losing $0^{s},3$. Barometer 29,70 Inches.

Temp.	Time of co-incidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Obs. Vibrations in 24 hours.	Corr. for Arc.	Vibrations in 24 hours.
°								
65,0	10 50 9	1,28						
	59 8	1,21	1,24	539			2,53	
	11 8 8	1,14	1,17	540			2,25	
	17 8	1,09	1,11	540			2,02	
	26 9	1,03	1,06	541			1,84	
	35 10	0,99	1,01	541			1,67	
	44 10	0,93	0,96	540			1,51	
	53 11,5	0,89	0,91	541,5			1,36	
	12 2 13	0,83	0,86	541,5			1,21	
	11 14	0,80	0,81	541			1,07	
66,2	20 17,5	0,77	0,78	543,5			0,99	
65,6	Mean			540,85	538,85	86080,21	1,65	86081,86

April 28, by Mr. Rumker.

Clock losing, $0^s,5$. Barometer, 29,82 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Corrections for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
65,7	11 26 34	1,22						
	35 33,5	1,17	1,19	539,5			2,31	
	44 34,5	1,08	1,12	541			2,03	
	53 34,5	1,04	1,06	540			1,84	
	12 2 36,5	0,99	1,02	542			1,70	
	11 38,2	0,93	0,96	541,7			1,51	
	20 38,6	0,90	0,91	540,4			1,36	
	29 42,2	0,82	0,86	543,6			1,21	
	38 43,2	0,79	0,81	541			1,07	
	47 45,1	0,73	0,76	542			0,95	
66,1	56 49,0	0,70	0,71	544			0,83	
65,9	Mean			541,52	539,52	86080,39	1,48	86081,87

April 29, by Mr. Rumker.

Clock losing $0^s,7$. Barometer 29,84 Inches.

65,0	11 17 44,5	1,31						
	26 44,2	1,23	1,27	539,7			2,64	
	35 44,5	1,17	1,20	540,3			2,35	
	44 45,0	1,12	1,14	540,5			2,13	
	53 44,0	1,04	1,08	539,0			1,92	
	12 2 45,0	0,98	1,01	541,2			1,67	
	11 48,3	0,93	0,95	543,1			1,48	
	20 49,1	0,88	0,90	540,8			1,33	
	29 51,0	0,83	0,85	542,0			1,18	
	28 50,7	0,79	0,81	539,7			1,07	
65,6	37 52,2	0,74	0,76	541,5			0,95	
65,3	Mean			540,78	538,78	86079,76	1,67	86081,43

April 30, by Sir Thomas Brisbane.

Clock losing $0^s,5$. Barometer 30,20 Inches.

64,0	11 8 16	1,38						
	17 17	1,31	1,34	541			2,95	
	26 18	1,23	1,27	541			2,64	
	35 19	1,16	1,19	541			2,33	
	44 19	1,10	1,13	540			2,10	
	53 21	1,03	1,06	542			1,84	
	12 2 23	0,99	1,01	542			1,67	
	11 25	0,92	0,95	542			1,48	
	20 27	0,89	0,90	542			1,33	
	29 30	0,83	0,86	543			1,21	
64,9	38 32,5	0,80	0,81	542,5			1,08	
64,4	Mean			541,65	539,65	86080,47	1,86	86082,33

May 1, by Sir THOMAS BRISBANE.

Clock losing $0^s,3$. Barometer 30,07 Inches.

Temp.	Time of coincidence.	Arc of Vibration,	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Corrections for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
62,8	11 24 54	1,32						
	33 54	1,24	1,28	540			2,69	
	42 55	1,18	1,21	541			2,39	
	51 56	1,11	1,14	541			2,31	
	12 0 57,5	1,04	1,07	541,5			1,88	
	10 1	1,00	1,02	543,5			1,70	
	19 2	0,94	0,97	541			1,54	
	28 3,5	0,90	0,92	541,5			1,38	
	37 5,5	0,84	0,87	542			1,24	
63,6	46 9	0,80	0,82	543,5			1,10	
63,2	Mean			541,66	539,66	86080,67	1, 80	86082,47

Vibrations of the Pendulum at London.

Date.	Barom.	Therm.	Vibrations in 24 hours.	Correction for Temp.	Vibrations in 24 hours at 60 degrees.
	Inches	°		+	
April 25	29,62	63,7	86082,19	1,57	86083,76
26	29,70	65,1	86081,61	2,16	86083,77
27	29,70	65,6	86081,86	2,37	86084,23
28	29,82	65,9	86081,87	2,50	86084,37
29	29,84	65,3	86081,43	2,24	86083,67
30	30,20	64,4	86082,33	1,86	86084,19
May 1	30,07	63,2	86082,47	1,35	86083,82
Mean	29,85	64,74			86083,97

The mean height of the barometer being 29,85 inches, and that of the thermometer 64°,74 during the preceding experiments, we have 6,40 for the correction for the buoyancy of

the atmosphere, and 86090,17 (the same as the former result), for the number of vibrations which would be made by the pendulum at London in latitude 51° 31′ 8″,4 in a mean solar day at the temperature of 60°, and in a vacuum.

From a slight sketch and an accompanying description, every care has evidently been taken by Sir Thomas Brisbane, that no suspicion of a want of solidity should attach to the support built for the pendulum at Paramatta. This is constructed of solid masonry, upwards of five feet in width, on a foundation five feet below the level ground, and is insulated from the floor. The square part of the cast iron frame, to which the bell-metal support of the pendulum is attached, is let into a stone nearly six inches thick, which surmounts the whole mass.

The following are the details of the observations made by Sir Thomas Brisbane at Paramatta, the latitude being 33° 48′ 43″ south, and the longitude 151° 0′ 15″ East from Greenwich.

FIRST SERIES.

August 27th, 1822.
Clock going mean time.

Paramatta.
Barometer 29,55 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
53,0	7 15 56	1,52	1,50	448			3,70	
	23 24	1,48	1,45	449			3,45	
	30 53	1,42	1,39	451			3,17	
	38 24	1,37	1,35	449			2,98	
	45 53	1,34	1,31	449			2,82	
	53 22	1,29	1,26	450			2,61	
	8 0 52	1,23	1,21	451			2,40	
	8 23	1,20	1,18	449			2,28	
	15 52	1,17	1,15	448			2,17	
	23 20	1,13	1,10	449			1,98	
53,4	30 49	1,08						
53,2			Mean	449,3	447,3	86015,40	2,76	86018,16

August 28, 1822. Paramatta.

Clock going mean time. Barometer 28,60 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
51,3	7 14 40	1,31						
	22 10	1,26	1,28	450			2,68	
	29 40	1,20	1,23	450			2,48	
	37 10	1,16	1,18	450			2,28	
	44 40	1,10	1,13	450			2,10	
	52 10,5	1,07	1,08	450,5			1,92	
	59 41	1,02	1,04	450,5			1,77	
	8 7 10	1,01	1,02	449			1,71	
	14 40,5	0,95	0,98	450,5			1,58	
	22 10	0,90	0,92	449,5			1,39	
52,7	29 42	0,88	0,89	452			1,30	
52,0			Mean	450,2	448,2	86016,18	1,92	86018,10

August 29. Barometer 29,70 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
52,0	6 59 41	1,31						
	7 7 10	1,25	1,28	449			2,69	
	14 40	1,20	1,22	450			2,45	
	22 10	1,17	1,18	450			2,28	
	29 40	1,12	1,14	450			2,14	
	37 9,5	1,09	1,10	449,5			1,99	
	44 40,5	1,05	1,07	451			1,88	
	52 12	0,99	1,02	451,5			1,71	
	59 42	0,96	0,97	450			1,54	
	8 7 13,5	0,92	0,94	451,5			1,45	
52,2	14 45,5	0,89	0,90	452			1,33	
52,1			Mean	450,5	448,5	86016,43	1,95	86018,38

August 30. Barometer 29,67 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
53,0	7 22 13,5	1,13						
	29 42,5	1,09	1,11	449			1,98	
	37 11,5	1,06	1,07	449			1,88	
	44 41,5	1,02	1,04	450			1,77	
	52 10	0,98	1,00	448,5			1,64	
	59 38	0,93	0,95	448			1,48	
	8 7 6	0,91	0,92	448			1,39	
	14 38	0,87	0,89	452			1,30	
	22 9,5	0,83	0,85	451,5			1,18	
	29 41,5	0,81	0,82	452			1,10	
54,0	37 13	0,78	0,79	451,5			1,02	
53,5			Mean	449,9	447,9	86015,92	1,47	86017,39

August 31.						Barometer 29,52 Inches.		
Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	Number of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
57,7	10 6 59	0,92						
	14 26	0,88	0,90	447			1,33	
	21 55,5	0,83	0,85	449,5			1,18	
	29 22	0,82	0,82	446,5			1,10	
	36 52	0,80	0,81	450			1,08	
	44 24	0,77	0,78	452			1,00	
	51 53,5	0,73	0,75	449,5			0,92	
	59 24	0,71	0,72	450,5			0,85	
	11 6 56	0,69	0,70	452			0,80	
	14 26	0,68	0,68	450			0,76	
58,8	21 58	0,63	0,65	452			0,69	
58,2		Mean		449,9	447,9	86015,92	0,97	86016,89
September 1.						Barometer 29,86 Inches.		
55,2	7 31 58	1,07						
	39 30	1,03	1,05	452			1,81	
	46 56	1,01	1,02	446			1,71	
	54 26	0,98	0,99	450			1,61	
	8 1 54	0,93	0,95	448			1,48	
	9 20	0,89	0,91	446			1,36	
	16 45	0,87	0,88	445			1,27	
	24 10	0,84	0,85	445			1,18	
	31 35	0,82	0,83	445			1,13	
	39 3	0,79	0,80	448			1,05	
56,6	46 34	0,74	0,76	451			0,95	
55,9		Mean		447,6	445,6	86013,94	1,36	86015,20
September 2.						Barometer 29,86 Inches.		
53,0	8 17 25	0,78						
	24 54	0,77	0,77	449			0,97	
	32 26	0,75	0,76	452			0,94	
	39 56,3	0,72	0,73	450,3			0,87	
	47 26	0,69	0,70	449,7			0,80	
	55 00	0,67	0,68	454			0,76	
	9 2 30	0,64	0,65	450			0,69	
	10 1,5	0,61	0,62	451,5			0,63	
	17 33,5	0,58	0,59	452			0,57	
	25 06	0,56	0,57	452,5			0,53	
55,0	32 38	0,55	0,55	452			0,49	
54,0		Mean		451,3	449,3	86017,01	0,73	86017,74

September 3. Barometer 29,73 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	Number of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
57,5	7 5 16,5	0,97	0,94	446,5			1,45	
	12 43	0,92	0,90	448			1,32	
	20 11	0,89	0,86	449			1,21	
	27 40	0,84	0,82	450			1,10	
	35 10	0.81	0,80	451,5			1,05	
	42 41,5	0,79	0,76	450			0,95	
	50 11,5	0,74	0,72	451			0,85	
	57 42,5	0,71	0,70	451,5			0,80	
	8 5 14	0,69	0,67	451,5			0,73	
	12 45,5	0,65	0,63	450,5			0,65	
58,2	20 16	0,62						
57,8		Mean		450,0	448	86016,00	1,01	86017,01

September 4. Barometer 29,80 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	Number of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
55,1	7 2 58	0,83	0,82	446,5			1,10	
	10 24,5	0,81	0,80	449,5			1,05	
	17 54	0,79	0,78	450			1,00	
	25 24	0,78	0,75	450,5			0,92	
	32 54,5	0,73	0,72	451			0,85	
	40 25,5	0,71	0,70	451			0,80	
	47 56,5	0,70	0,69	451			0,78	
	55 27,5	0,69	0,68	451			0,76	
	8 2 58,5	0,67	0,64	451,5			0,67	
	10 30	0,62	0,61	450,5			0,61	
55,9	18 00,5	0,60						
55,5		Mean		450,3	448,3	86016,25	0,85	86017,10

September 4. Barometer 29,64 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	Number of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
60,0	0 17 16,5	0,99	0,96	445,5			1,51	
	24 42	0,94	0,92	445			1,39	
	32 07	0,91	0,89	443			1,30	
	39 30	0,87	0,84	448			1,16	
	46 58	0,82	0,80	450			1,05	
	54 28	0,79	0,76	448,5			0,95	
	1 1 56,5	0,74	0,73	450			0,87	
	9 26,5	0,72	0,71	449,5			0,83	
	16 56	0,70	0,68	449			0,76	
	24 25	0,67	0,65	449,5			0,69	
61,0	31 54,5	0,63						
60,5		Mean		447,8	445,8	86014,12	1,05	86015,17

From the above details are obtained the following results,

Vibrations of the Pendulum at Paramatta.

1st Series.

Date.	Barom.	Thermometer.	Vibrations in 24 hours.	Correction for Temperature.	Vibrations in 24 hours at 60 degrees.
	Inches	°			
Aug. 27	29,55	53,2	86018,16	— 2,88	86015,28
28	26,60	52,0	86018,10	3,38	86014,72
29	29,70	52,1	86018,38	3,34	86015,04
30	29,67	53,5	86017,39	2,75	86014,64
31	29,52	58,2	86016,89	0,76	86016,13
Sept. 1	29,86	55,9	86015,20	1,73	86013,47
2	29,86	54,0	86017,74	2,54	86015,20
3	29,73	57,8	86017,01	0,93	86016,08
4	29,80	55,5	86017,10	1,90	86015,20
	29,64	60,5	86015,17	+ 0,21	86015,19
Mean	29,69	55,27			85015,10

The mean height of the barometer during these experiments was 29,69 inches, and the mean temperature 55°,27, from which, and the specific gravity of the pendulum, we have 6,49 for the correction on account of the buoyancy of the atmosphere.

Adding this to the mean number of vibrations before found, we obtain 86021,59 for the number of vibrations which would be made by the pendulum at 60° in a vacuum in a mean solar day.

In addition to the experiments already given, Sir Thomas Brisbane has forwarded another series made at Paramatta with the same pendulum, by Mr. Dunlop, a gentleman of whose zeal and scientific abilities Sir Thomas Brisbane expresses himself in the highest terms.

The following are the details.

SECOND SERIES.

August 31, 1822. Paramatta.
Clock going mean time. Barometer 29,55 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
58,0	1 19 8	1,45	1,41	461			3,26	
	26 49	1,37	1,33	438			2,90	
	34 7	1,30	1,26	450			2,60	
	41 37	1,23	1,19	449			2,32	
	49 6	1,16	1,13	449			2,08	
	56 35	1,10	1,08	442			1,91	
	2 3 57	1,07	1,05	449			1,80	
	11 26	1,04	1,02	441			1,70	
	18 47	1,00	0,98	450			1,57	
	26 17	0,96	0,94	450			1,44	
	33 47	0,92						
58,0	Mean			447,9	445,9	86014,20	2,16	86016,36
	September 1.					Barometer 29,87 Inches.		
57 0	0 40 45	0,94	0,92	447			1,38	
	48 12	0,90	0,88	446,5			1,26	
	55 38,5	0,87	0,85	447,5			1,18	
	1 3 6	0,83	0,80	449			1,04	
	10 35	0,78	0,76	448			0,94	
	18 3	0,75	0,73	451			0,87	
	25 34	0,72	0,70	449			0,80	
	33 3	0,69	0,68	450			0,75	
	40 33	0,67	0,65	451			0,69	
	48 4	0,63	0,61	450,5			0,61	
	55 34,5	0,60	0,58	449,5			0,55	
	2 3 4	0,57	0,56	451			0,51	
57, 5	10 35	0,55						
57,25	Mean			449,2	447,2	86015,32	0,88	86016,20

September 2. Barometer 29,71 Inches.

Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
56, 5	11 28 51	0,90	0,88	449			1,26	
	36 20	0,87	0,85	449			1,18	
	43 49	0,83	0,81	451			1,07	
	51 20	0,80	0,78	451			0,99	
	58 51	0,77	0,75	449,5			0,92	
	0 6 20,5	0,73	0,71	450,5			0,82	
	13 51	0,70	0,68	452			0,75	
	21 23	0,67	0,65	451			0,69	
	28 54	0,64	0,62	450,5			0,63	
	36 24,5	0,61	0,60	451,5			0,59	
	43 56	0,59	0,58	451			0,55	
	51 27	0,57	0,56	450,5			0,51	
	58 57,5	0,55	0,54	450,5			0,47	
	1 6 28	0,53	0,52	450,5			0,44	
	13 58,5	0,51	0,50	450,5			0,41	
53, 8	21 29	0,49						
55,15	Mean			450,5	448,5	86016,43	0,75	86017,18
September 3.				Barometer 29,80 Inches.				
59,6	10 50 12	0,92	0,90	448			1,32	
	57 40	0,89	0,87	449			1,23	
	11 5 9	0,85	0,83	450			1,13	
	12 39	0,82	0,80	449			1,04	
	20 8	0,78	0,76	450			0,94	
	27 38	0,74	0,72	450			0,85	
	35 8	0,71	0,69	450			0,78	
	42 38	0 68	0,66	450			0,71	
60,0	50 8	0,65	0,63	450			0,65	
	57 38	0,62	0,61	449			0,61	
	0 5 7	0,60	0,59	450			0,57	
	12 37	0,58	0,56	449			0,51	
	20 6	0,55	0,53	449			0,46	
	27 35	0,52	0,51	449,5			0,42	
	35 4,5	0,50	0,49	449,5			0,39	
	42 34	0,48	0,47	449,5			0,36	
61,0	50 3,5	0,47						
60,2	Mean			449,5	447,5	86015,57	0,75	86016,32

September 4.				Barometer 29,74 Inches.				
Temp.	Time of coincidence.	Arc of Vibration.	Mean Arc.	Interval in Seconds.	No. of Vibrations.	Observed Vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
56, 5	9 4 1	0,88						
	11 29	0,84	0,86	448			1,20	
	18 58	0,82	0,83	449			1,12	
	26 27	0,78	0,80	449			1,04	
	33 57	0,76	0,77	450			0,97	
	41 27	0,71	0,73	450			0,87	
	48 57,5	0,69	0,70	450,5			0,80	
	56 28	0,65	0,67	450,5			0,73	
	10 3 59	0,64	0,64	451			0,67	
	11 30,5	0,60	0,62	451,5			0,63	
	19 2	0,60	0,60	451,5			0,59	
58, 0	26 33	0,54	0,57	451			0,53	
57,25	Mean			450,2	448,2	86016,17	0,83	86017,00
September 4.				Barometer 29,64 Inches.				
58, 0	10 39 22	0,85						
	46 50	0,81	0,83	448			1,12	
	54 20	0,79	0,80	450			1,04	
	11 1 50	0,74	0,76	450			0,94	
	9 20	0,72	0,73	450			0,87	
	16 50	0,69	0,70	450			0,80	
	24 21	0,67	0,68	451			0,75	
	31 51	0,63	0,65	450			0,69	
	39 21	0,61	0,62	450			0,63	
	46 51	0,58	0,59	450			0,57	
59, 7	54 20,5	0,57	0,57	449,5			0,53	
58,85	Mean			449,9	447,9	86015,92	0,79	86016,71
September 4.				Barometer 26,56 Inches.				
61, 8	2 15 10	0,95						
	22 36	0,92	0,93	446			1,42	
	30 02	0,89	0,90	446			1,32	
	37 29	0,86	0,87	447			1,24	
	44 59	0,82	0,84	450			1,15	
	52 27	0,78	0,80	448			1,04	
	59 56	0,74	0,76	449			0,94	
	3 7 25	0,71	0,72	449			0,85	
	14 52	0,68	0,69	447			0,78	
	22 22	0,65	0,66	450			0,71	
62, 5	29 49	0,62	0,63	447			0,65	
62,15	Mean			447,9	445,9	86014,20	1,01	86015,21

From the above we have the following results.

Vibrations of the Pendulum at Paramatta. (2*d. Series.*)

Date.	Barometer.	Thermometer.	Vibrations in 24 hours.	Correction for Temperature.	Vibrations in 24 hours at 60 degrees.
	Inches.	°			
Aug. 31	29,55	58, 0	86016,36	—0,85	86015,51
Sept. 1	29,87	57,25	86016,20	—1,16	86015,04
2	29,71	55,15	86017,18	—2,05	86015,13
3	29,80	60, 2	86016,32	+0,09	86016,41
4	29,74	57,25	86017,00	—1,16	86015,84
—	29,64	58,85	86016,71	—0,47	86016,24
—	29,56	62,15	86015,21	+0,89	86016,10
Mean	29,75	58,41			86015,75

The barometer being at 29,75 inches, and the thermometer at 58,41 during the experiments, we have 6,46 for the buoyancy of the atmosphere, which, added to the mean number of vibrations, gives 86022,21 for the number of vibrations which would be made by the pendulum in a mean solar day at 60°, and in a vacuum.

The height of the station at Paramatta, above mean *high* water, is stated by Sir T. Brisbane to be seventy-seven feet, and that of the station at London being eighty-three feet above *low* water, it could only introduce error to attempt any correction for a difference which has not yet been accurately ascertained.

If the number of vibrations resulting from Sir Thomas Brisbane's experiments at Paramatta be compared with the mean number of vibrations made by the pendulum at London, we shall have 39,07696 inches for the length of the pendulum vibrating seconds at Paramatta; ,0052704 for the diminution of gravity from the pole to the equator; and $\frac{1}{295,84}$ for the

resulting compression; the length of the pendulum vibrating seconds at London being taken at 39,13929 inches.

The experiments at Paramatta being compared with those made by me at Unst, in latitude 60° 45′ 28″ north, give ,0053605 for the diminution of gravity from the pole to the equator, and $\frac{1}{303,95}$ for the resulting compression.

If Mr. DUNLOP'S experiments at Paramatta be compared with those made at London, we obtain 39,07751 for the length of the seconds pendulum at Paramatta, ,0052238 for the diminution of gravity from the pole to the equator, and $\frac{1}{291,83}$ for the compression. Or, comparing Mr. DUNLOP'S experiments with those made at Unst, we have ,0053292 for the diminution of gravity from the pole to the equator, and $\frac{1}{301,09}$ for the resulting compression.

The compressions here deduced must not as yet be deemed conclusive, for it is well known that a very small alteration in the number of vibrations made by the pendulum would occasion a considerable difference in the fraction indicating the compression. The indefatigable zeal of Sir THOMAS BRISBANE, will, however, no doubt soon furnish additional data.

I have the honour to be,
My dear Sir,
very sincerely yours,
HENRY KATER.

London,
June, 1823.

P. S. I may here take the opportunity of correcting an error in the "Account of Experiments for determining the variation in the length of the Pendulum vibrating seconds at the principal stations of the Trigonometrical Survey of Great Britain."

In the first series of observations made with the repeating circle for the latitude of Clifton, 1′ 41″,6 has been applied as the correction for the level instead of 141″,6 = 2′ 21″, 6. The resulting latitude, when the proper correction is made is 53° 27′ 44″, 94 instead of 53° 27′ 40″, 94, and the greatest difference between the five independent latitudes of Clifton 3″,48 instead of 5″,24.

XXIII. *Observations and Experiments on the daily variation of the Horizontal and Dipping Needles under a reduced directive power. By* PETER BARLOW, *Esq. F.R.S. of the Royal Military Academy. Communicated by* DAVIES GILBERT, *Esq. V.P.R.S.*

Read June 12, 1823.

IT is now just a century since Mr. GRAHAM discovered the daily change in the variation of the horizontal needle, subsequent to which time numerous observations have been made on the same subject by WARGENTIN, CANTON, GILPIN, Colonel BEAUFOY and others, which have all confirmed, with certain shades of variety, the general fact as first described by the ingenious philosopher above named.

The actual daily change however is so small, even in the horizontal needle, that it can only be detected with the most careful observations and with the most delicate instruments; and in the dipping needle that change, if any, is so extremely minute, as hitherto to have escaped observation: for it was only in the year 1820, that the Royal Academy of Sciences of Copenhagen proposed the determination of this motion, on satisfactory experiments, as the prize subject for that year; but the prize, I understand, has never been adjudged, no satisfactory communication having been received.

Under this difficulty of observation it occurred to me, that it would be possible to increase this deviation on both needles, so as to render it distinctly observable, by reducing the directive power of the needle by means of one or two magnets,

properly disposed to mask, at least in part, the terrestrial influence; a method which has been long practised by mineralogists and others, when the object has been to detect minute attractions. I expected by this means that the cause, whatever it might be, that produces the daily variation, would exhibit itself in an encreased degree, and thereby render the results more perspicuous, and fix with more precision than has hitherto been done, the time of change and moment of maximum effect.

Suppose, for example, that a finely suspended horizontal needle, under the natural influence of the earth, makes one vibration in 2″, and that by masking the terrestrial influence by magnets properly adjusted, that time of vibration is encreased to 8″; then it would follow that the directive power was reduced to one sixteenth of the former, and consequently, that any lateral magnetic force acting upon the needle would produce an effect sixteen times greater than before; so that if the former were 12′, the new effect or deviation might be expected to amount to between three and four degrees, and therefore be such as to admit of distinct and satisfactory observation.

A course of experiments carried on for a few days, convinced me that my ideas were correct, and that we might, while the needle was kept in its natural meridian, or rather adjusted to that direction, produce a daily variation to almost any amount. I obtained, for instance, the first day, a maximum deviation of 3° 40′; the second, I encreased it by bringing up my magnets to 7°; the third day I reduced it to 2°, and so on. I found, also, that a very considerable daily change would exhibit itself with the north end held to the south, to

the east, west, and, in short, in any position at pleasure, at least within certain limits, which will be pointed out as we proceed.

For this it is only necessary, first, to deflect the needle by repulsion into any required position, and then, by means of another magnet, to modify its directive power, in the same way as when in its natural meridian. Or the same may be done by bringing two magnets with their contrary poles pointing inwards, and each opposite to the pole of the same name of the needle placed between them, and by a slight adjustment of the former to produce the deviation in question: or, which is perhaps still better, the opposing magnets may be brought into the actual direction of the dip, and then adjusted to produce the deflection required.

Having mentioned my ideas and first experiments to my colleague, Mr. CHRISTIE, and having expressed a wish that he would repeat them for the sake of verification, he very readily agreed to undertake a complete set, with the needle in its natural meridian, by means of a very delicate compass, and an apparatus he had employed for other experiments, and which admitted of his bringing his neutralizing magnets very exactly into the line of the dip. In the mean time I proposed to undertake the observations on the dipping needle, and on the horizontal needle in different directions; viz. with its north end pointing to the south, east, west, &c. Having, however, met with some embarrassment in the commencement, and having employed, in consequence, a longer time in the observations than I had anticipated, Mr. CHRISTIE, after having finished his observation in the meridian, continued them at other points, and has thereby detected several curi-

ous and minute peculiarities, which, with his other experiments, will, I hope, accompany this Memoir.

Account of the observations made on the daily variation of the horizontal needle in various directions.

My first experiments, as I have already stated, were only matters of trial, from which I had merely ascertained that the idea I had formed was practicable, and that in certain situations the needle had certain directions of motion, but I had obtained no numerical results. Having, however, provided myself with a needle proper for the purpose, very delicate and light, and eight inches and a half in length, I began, towards the end of March, to register the amount of the daily change at every hour, or half hour, from morning to night; my son taking the observations during my occasional absence.

My first observation in the new series was made with the north end of the needle pointing to the west, balanced in that position with two magnets placed to the southward attracting each extremity; the directive power was considerably reduced, and I obtained a maximum deviation of 3° 15′; which happened at about eleven o'clock in the forenoon, and from which time the variation decreased to a late hour in the evening. The needle was kept in this position for three days, with some change of directive power, but the character of the results, as to the direction of motion, the times of commencement and maximum, &c. were of precisely the same nature, but the amount was more or less, according to the directive power left upon the needle.

Having, however, after a few days removed my apparatus

from the room in which the experiments had hitherto been made, into a bower in my garden, and having detected a remarkable difference in the results obtained in these two situations, I determined to commence the experiments *de novo* in this latter spot, which was at least thirty yards distant from any building; and afterwards to examine the cause of the difference in question. This examination is reported in the conclusion of this article.

All the following experiments, therefore, will be understood to have been made in the open air, proper precautions having been taken to ensure stability. The results are arranged according to the direction in which the needle pointed; and the character of the daily deviation is distinguished as follows. The reader must conceive himself as facing the north end of the needle; then, when that end passes to his right hand the deviation is marked (+), and when to his left hand (—); the bearing at six o'clock in the morning being marked zero.

TABLE *of observed daily Variations.*

North end of Needle to North.				North end to the South.			
Hours.	1st Day.	2nd Day.	3rd Day.	Hours.	1st Day.	2nd Day.	3rd Day.
h. m.	° ′	° ′	° ′	h. m.	° ′	° ′	No observation on the 3rd day.
6 0	0 0	0 0	0 0	6 0	0 0	0 0	
7 0	—0 10	+0 5	—0 5	7 0	—0 30	—0 15	
8 0	+0 10	+0 15	+0 20	8 0	—0 40	—0 45	
9 0	+0 30	+0 50	+1 0	9 0	—0 55	—1 5	
10 0	—	+1 0	—	10 0	—	—1 25	
10 30	+1 5	+1 0	+1 50	10 30	—1 10	—1 50	
11 0	+1 45	+1 30	+2 10	11 0	—1 10	—1 50	
11 30	+2 5	+1 45	+2 20	11 30	—1 10	—2 0	
12 0	+2 5	+1 45	+3 0	12 0	—	—	
1 0	+2 50	+1 50	+3 10	1 0	—1 15	—2 20	
2 0	+2 40	+1 30	+2 45	2 0	—1 5	—2 20	
3 0	+2 30	+1 30	+2 45	3 0	—1 0	—2 20	
4 0	+2 0	+1 30	—	4 0	—	—1 30	
5 0	+1 30	+1 0	+1 40	5 0	—	—1 10	
6 0	+1 30	—	—	6 0	—0 45	—1 10	
7 0	+1 10	—	+1 0	7 0	—0 35	—	
8 0	+1 0	—	+0 45	8 0	—	—0 45	
9 0	+1 0	—	+0 30	9 0	—0 15	—0 30	
10 0	+0 45	+0 15	+0 30	10 0	—	—0 30	
11 0	+0 10	—	—	11 0	—	—	

North end to N. N. E.				North end to S. S. W.			
Hours.	1st Day.	2nd Day.	3rd Day.	Hours.	1st Day.	2nd Day.	3rd Day.
h. m.	° ′	° ′	No observation on the 3rd day.	h. m.	° ′	° ′	No observation on the 3d day.
6 0	0 0	0 0		6 0	0 0	0 0	
7 0	—0 5	0 0		7 0	—0 10	—0 20	
8 0	+0 15	+0 20		8 0	—0 30	—0 40	
9 0	+0 40	+0 45		9 0	—0 45	—1 0	
10 0	+1 0	—		10 0	—1 0	—1 20	
10 30	+1 5	—		10 30	—1 10	—	
11 0	+1 40	+1 45		11 0	—1 30	—	
11 30	+1 40	+2 0		11 30	—1 30	—2 0	
12 0	+2 0	—		12 0	—1 30	—2 20	
1 0	+2 10	—		1 0	—2 0	—2 30	
2 0	+2 10	+2 0		2 0	—2 0	—	
3 0	—	+1 50		3 0	—	—	
4 0	—	—		4 0	—	—1 30	
5 0	+1 30	+1 45		5 0	—	—	
6 0	+1 35	—		6 0	—1 10	—1 15	
7 0	—	+0 55		7 0	—1 0	—0 55	
8 0	+1 10	+0 55		8 0	—1 0	—0 50	
9 0	+0 50	+0 45		9 0	—1 0	—	
10 0	+0 30	+0 0		10 0	—0 30	—0 40	
11 0	—0 0	—0 10		11 0	—	—	

North end N. E.

Hours.	1st Day.	2nd Day.	3rd Day.
h. m.	° ′	° ′	No observation on the 3rd day.
6 0	0 0	0 0	
7 0	+0 10	+0 15	
8 0	+0 30	+0 15	
9 0	+0 45	+0 50	
10 0	+1 0	—	
10 30	+1 20	—	
11 0	+1 50	+1 30	
11 30	+1 50	—	
12 0	+2 0	+2 10	
1 0	+2 0	—	
2 0	+1 45	+1 15	
3 0	—	+1 15	
4 0	—	+1 15	
5 0	—	+1 0	
6 0	+0 45	+0 45	
7 0	+0 45	—	
8 0	+0 30	+0 25	
9 0	+0 10	+0 25	
10 0	—0 5	0 0	
11 0	—0 5	—	

South end S. W.

Hours.	1st Day.	2nd Day.	3rd Day.
h. m.	° ′	° ′	No observation on the 3rd day.
6 0	0 0	0 0	
7 0	—0 20	0 0	
8 0	—0 35	—0 20	
9 0	—0 45	—0 30	
10 0	—0 45	—0 40	
10 30	—1 10	—	
11 0	—1 10	—	
11 30	—1 10	—1 30	
12 0	—1 50	—1 30	
1 0	—1 50	—1 30	
2 0	—1 40	—1 15	
3 0	—	—	
4 0	—	—1 0	
5 0	—	—1 0	
6 0	—0 45	—0 50	
7 0	—0 30	—	
8 0	—0 30	—	
9 0	—0 0	—	
10 0	+0 5	—	
11 0	+0 5	+0 10	

North end E. N. E.

Hours.	1st Day.	2nd Day.	3rd Day.
h. m.	° ′	° ′	No observation on the 3rd day.
6 0	0 0	0 0	
7 0	+0 10	+0 40	
8 0	+0 45	+0 55	
9 0	+1 15	+1 30	
10 0	+2 0	+2 10	
10 30	+2 10	+2 40	
11 0	—	—	
11 30	—	—	
12 0	—	—	
1 0	+2 0	+2 30	
2 0	+2 0	+2 10	
3 0	—	—	
4 0	—	+1 40	
5 0	—	+1 0	
6 0	—	—	
7 0	+1 10	—	
8 0	+0 40	+0 50	
9 0	+0 20	+0 20	
10 0	0 0	—0 5	
11 0	—0 15	—	

North end W. S. W.

Hours.	1st Day.	2nd Day.	3rd Day.
h. m.	° ′	° ′	No observation on the 3rd day.
6 0	—0 0	0 0	
7 0	—0 30	—0 40	
8 0	—1 0	—	
9 0	—1 0	—	
10 0	—1 45	—2 0	
10 30	—2 25	—2 30	
11 0	—2 50	—2 45	
11 30	—2 50	—	
12 0	—2 50	—	
1 0	—2 10	2 10	
2 0	—	—	
3 0	—	2 15	
4 0	—	—	
5 0	—1 30	2 0	
6 0	—	—	
7 0	—	—	
8 0	—1 0	1 30	
8 0	—1 0	1 15	
10 0	—0 25	0 50	
11 0	+0 5	—0 20	

North end East.				North end West.			
Hours.	1st Day.	2nd Day.	3rd Day.	Hours.	1st Day.	2nd Day.	3rd Day.
h. m.	° ′	° ′	No observation on the 3rd day.	h. m.	° ′	° ′	° ′
6 0	0 0	0 0		6 0	0 0	0 0	0 0
7 0	+1 30	+0 30		7 0	—0 15	—0 0	—0 10
8 0	+1 50	+1 0		8 0	—1 5	—0 5	—1 0
9 0	+2 0	+1 15		9 0	—1 35	—0 35	—1 20
10 0	+3 15	+1 35		10 0	—2 30	—0 50	—2 15
10 30	—	+2 0		10 30	—2 55	—1 25	—
11 0	+3 15	+2 0		11 0	—3 5	—1 40	—2 45
11 30	—	+2 0		11 30	—3 5	—1 40	—2 45
12 0	+3 15	+2 0		12 0	—2 25	—1 20	—2 0
1 0	+3 10	—		1 0	—	—1 0	—
2 0	+2 30	+1 30		2 0	—2 30	—0 40	—2 0
3 0	+1 30	+1 30		3 0	—2 20	—0 35	—2 0
4 0	+1 20	+1 10		4 0	—2 10	—0 30	—1 45
5 0	+0 45	+1 0		5 0	—2 10	—0 25	—1 0
6 0	+0 45	+1 0		6 0	—2 0	—0 20	—
7 0	+0 10	+1 10		7 0	—1 55	—0 10	—0 30
8 0	+0 5	+0 50		8 0	—1 35	+0 5	—0 30
9 0	+0 0	+0 30		9 0	—	+0 20	—
10 0	—0 5	+0 5		10 0	—0 55	+0 25	—
11 0	—	—		11 0	—	—	—

North end E. S. E,				North end W. N. W.			
Hours.	1st Day.	2nd Day.	3rd Day.	Hours.	1st Day.	2nd Day.	3rd Day.
h. m.	° ′	° ′	° ′	h. m.	° ′	° ′	No observation on the 3rd day.
6 0	—	0 0	0 0	6 0	—	0 0	
7 0	0 0	+0 5	+0 10	7 0	0 0	0 0	
8 0	+0 10	+0 15	+0 30	8 0	—0 20	—0 15	
9 0	+0 20	+0 30	—	9 0	—0 40	—	
10 0	+0 35	+0 35	—	10 0	—0 50	—0 20	
10 30	—	+0 45	—	10 30	—	—	
11 0	+0 40	—	—	11 0	—	—	
11 30	+0 40	+0 55	+1 0	11 30	—	—	
12 0	+0 40	—	+0 55	12 0	—0 55	—0 22	
1 0	+0 35	+0 50	+0 40	1 0	—0 55	—0 30	
2 0	+0 35	+0 55	—	2 0	—0 50	—0 30	
3 0	+0 35	+0 45	—	3 0	—0 50	—	
4 0	+0 35	+0 45	—	4 0	—	—0 30	
5 0	+0 30	—	—	5 0	—	—	
6 0	+0 25	+0 30	—	6 0	—	—	
7 0	+0 15	+0 30	+0 20	7 0	—0 30	0 0	
8 0	+0 5	+0 10	+0 10	8 0	—0 20	—	
9 0	—0 5	+0 5	+0 0	9 0	—0 5	+0 10	
10 0	—0 5	+0 0	—0 5	10 0	+0 5	+0 15	
11 0	—	—	—0 5	11 0	—	—	

North end S. E.			
Hours.	1st Day.	2nd Day.	3rd Day.
h. m.	° ′	° ′	
6 0	0 0	0 0	No observation on the 3rd day.
7 0	+0 10	0 0	
8 0	+0 20	+0 25	
9 0	+0 35	+0 25	
10 0	+0 45	+0 40	
10 30	+1 0	+1 10	
11 0	+1 5	+1 10	
11 30	+1 5	—	
12 0	—	—	
1 0	+1 10	—	
2 0	—	+1 0	
3 0	—	+1 0	
4 0	—	—	
5 0	+0 40	+0 55	
6 0	+0 40	+0 45	
7 0	—	—	
8 0	—	—	
9 0	+0 20	+0 15	
10 0	+0 10	—	
11 0	—	+0 5	

North end N. W.			
Hours.	1st Day.	2nd Day.	3rd Day.
h. m.	° ′	° ′	
6 0	0 0	0 0	No observation on the 3rd day.
7 0	—0 10	—0 5	
8 0	—0 15	—0 20	
9 0	—0 30	—0 30	
10 0	—0 45	—	
10 30	—0 50	—1 10	
11 0	—0 55	—1 0	
11 30	—	—1 0	
12 0	—0 55	—	
1 0	—0 50	—	
2 0	—0 50	—0 45	
3 0	—0 50	—	
4 0	—	—0 45	
5 0	—0 50	—	
6 0	—	—0 40	
7 0	—0 45	—	
8 0	—0 30	—0 30	
9 0	—	—	
10 0	—0 5	—0 15	
11 0	—0 5	—	

North end S.S.E.½ South.*			
Hours.	1st Day.	2nd Day.	3rd Day.
h. m.			
6 0	Very little daily variation.	No perceptible motion.	No motion to 2 o'clock this day. The needle then removed.
7 0			
8 0			
9 0			
10 0			
10 30			
11 0			
11 30			
12 0			
1 0			
2 0			
3 0			
4 0			
5 0			
6 0			
7 0			
8 0			
9 0			
10 0			
11 0			

North end N. N. W.			
Hours.	1st Day.	2nd Day.	3rd Day.
h. m.	° ′	° ′	
6 0	0 0	0 0	No observation on the 3rd day.
7 0	+0 6	0 0	
8 0	—0 5	—0 10	
9 0	—0 25	—0 25	
10 0	—0 30	—0 30	
10 30	—0 45	—0 40	
11 0	—0 45	—0 55	
11 30	—1 0	—1 10	
12 0	—1 10	—	
1 0	—1 0	—1 10	
2 0	—	—1 5	
3 0	—1 0	—	
4 0	—1 0	—	
5 0	—	—0 45	
6 0	—	—0 40	
7 0	—0 50	—0 40	
8 0	—0 45	—0 20	
9 0	—0 30	—0 20	
10 0	—0 10	—	
11 0	—0 0	—	

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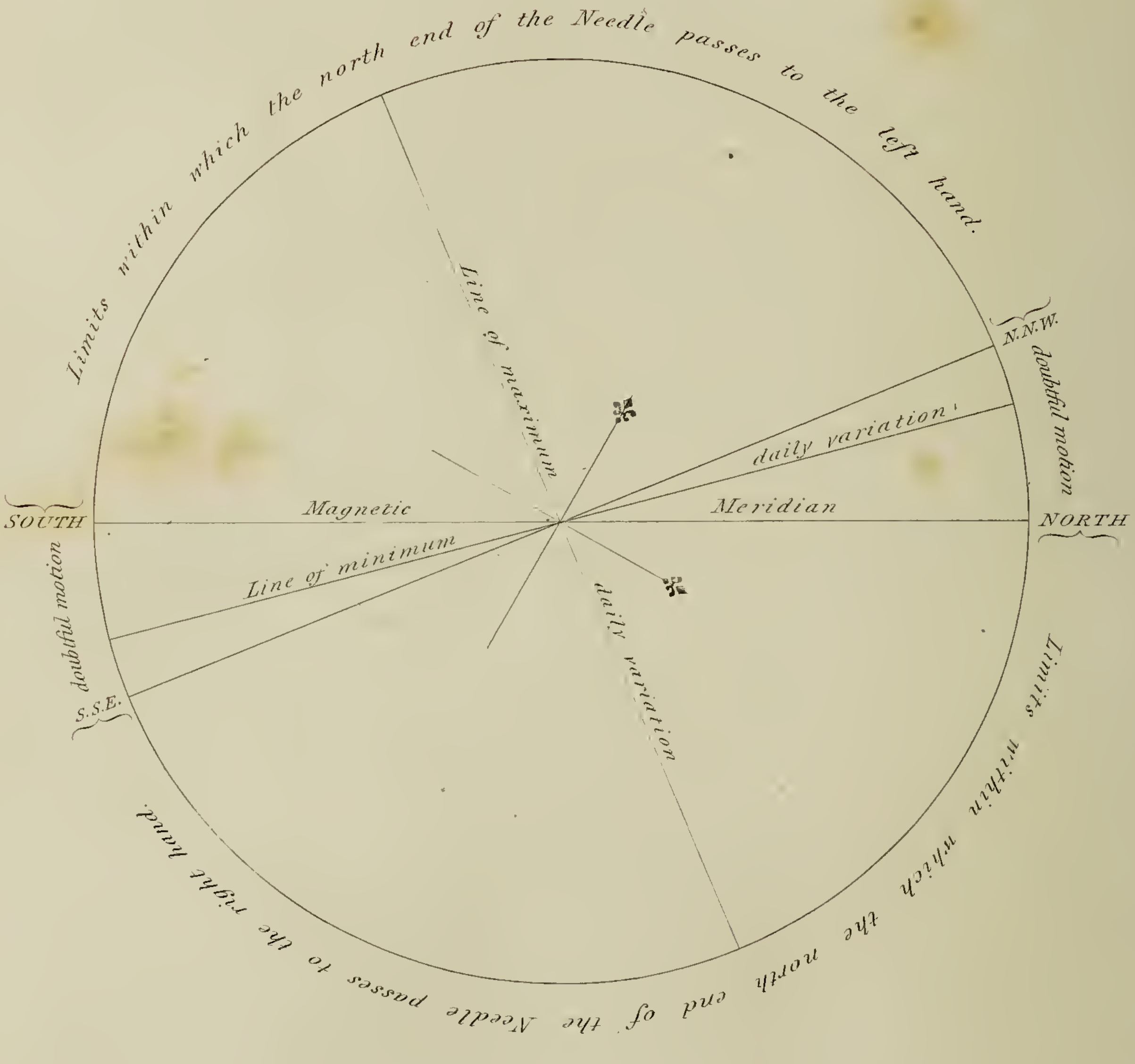
* The exact bearing of the needle in this case was N. 16° W. and S. 16° E.


Limits within which the north end of the Needle passes to the left hand.
N.N.W.
doubtful motion
daily variation
Line of maximum
SOUTH
Magnetic
Meridian
NORTH
Line of minimum
doubtful motion
S.S.E.
daily variation
Limits within which the north end of the Needle passes to the right hand.

J.^s Basire, sc.

From the above results, although the experiments were not made under such favourable circumstances as I could wish, we may draw some very curious, if not important conclusions; such, for instance, as the following. That while the north end of the needle is directed to any point from the south to NNW, its motion during the forenoon is towards the left hand; advancing therefore to some point between the NNW and north; and while it is directed towards any point between the north and SSE it passes to the right hand, that is still to some point between the north and NNW; the south end of the needle at the same time passing of course to some point between the south and SSE; so that it would seem that there ought to be some direction between those limits, viz. between the N and NNW, and the S and SSE, in which the daily motion is zero, or at least a minimum, (see Plate XXIII.): but whether this is a fixed direction during the year, or whether it has any vibratory motion as the sun changes its declination, or even during his daily course, is a question which cannot be decided without a much longer course of experiments than those I have here the honour to present.

It is also questionable, whether the direction of this line of no daily variation is the same in different parts of the world; a point on which I hope to obtain some information in the course of the present year. Mr. FOSTER,* of H. M. S. Griper,

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* I am already highly indebted to this Gentleman for the accurate and satisfactory observations he made during the recent voyage of H. M. S. Conway, under the command of Captain BASIL HALL, on the method I had the honour to propose for correcting the local attraction of vessels; and it is with great pleasure that I find he has been directed by the Admiralty to continue his attention to them in the present voyage of the Griper. My best thanks are also due to Captain HALL, for the facilities he afforded in the instance above mentioned, and for the judgment

having very obligingly undertaken to repeat my experiments at Spitzbergen, during the stay of the vessel at that place for the pendulum experiments; and from which we may hope to derive some interesting deductions, particularly in reference to the influence of the direction of the solar rays; for it is clear from the experiments reported in the preceding table, that the amount of the deviation does not entirely depend upon the moment when the heat of the sun is the greatest, as has been generally imagined; for the time of the maximum deviation varies from eleven o'clock in the morning to four o'clock in the afternoon, according to the direction in which the needle is pointed, and to other circumstances that will be mentioned in the conclusion of this article. Mr. Christie's observations are also of a kind to throw great light on this subject.

Another conclusion, which I think we are justified in drawing from the above experiments, is, that the daily change is not produced by a general deflection of the directive power of the earth, but by an increase and decrease of attraction of some point situated between the north and NNW, or between the south and SSE (see the figure above referred to); for I cannot conceive any other hypotheses that will account for two needles, situated as there shown, both approaching and both receding at the same time to and from the line of no daily variation; nor for the total suspension or equivocal vibratory motion of a needle when placed towards this direction.

I am sorry, that not foreseeing at the commencement of

with which he selected the most appropriate situations for submitting that method to the test of actual experiment.

my experiments, the length to which I should carry them, I did not, from the first, register the temperature and state of the atmosphere; for from certain notes of this kind made lately, it appears to me that the quantity of daily change depends in a greater degree on the intensity of the solar light, than on the mere temperature of the day; although it is certain, from some recent experiments by Mr. CHRISTIE, that the change of temperature of the air, during the day, has a much greater effect upon the intensity of action in the opposing magnets, than I could possibly have imagined.

On the daily variation of the dipping needle.

Notwithstanding my observations on the daily change of this instrument have not been so successful as those on the horizontal needle, yet it will be proper to say a few words on the subject of the experiments, although I do not intend, in the present instance, to give any numerical results; those I have obtained not being so uniform as I could wish, nor such as to justify their publication.

The instrument I employed was made by Messrs. W. and T. GILBERT: it was remarkably free and accurate, and certainly gave results with greater uniformity than any dipping needle I ever used. The needle was only six inches in length, a quarter of an inch broad, and very thin; it performed in the meridian forty-one vibrations in one hundred seconds, when under the usual terrestrial influence; and when masked and adjusted by two magnets placed in the line of the dip, it made only fifteen vibrations and a half in the same time; the power was therefore reduced about eight times.

It is not necessary to explain here the means that I employed, and the precautions I took to ensure stability; it

will be sufficient to observe, that I paid the utmost attention to this essential condition, and that I believe my want of success did not arise from any defect in this part of the process, but from the extreme delicacy of this instrument, and the consequent difficulty in adjusting it when under the influence of the neutralizing magnets. I tried its action for three weeks in the house, but the jarring of doors and other circumstances prevented me from drawing any conclusions; I then removed it to the garden, to a spot well protected by trees and shrubs, and fixed the entire apparatus to my garden wall, which is exactly in the magnetic meridian; and farther sheltered the whole in the best way I could from the effects of the wind and weather. Indeed the only inconvenience was that I could not leave the needle out in the night, and could therefore only notice what took place in the day time, and this, as I have said above, was not so uniform as I could have desired.

In general a motion commenced soon after the instrument was adjusted in the morning; but it was not of that gradual and progressive kind which indicated an uniformly increasing or decreasing power, as in the other instrument; it passed, for instance, suddenly from one half or quarter degree, to another more or less, and which sometimes in the course of the day would give a difference in the dip to the amount of a degree and a half, or even more, but I seldom saw in it a tendency to return; although when I vibrated it towards night, it commonly took up its morning position. I made these observations with the needle in various directions, viz. with the face of the instrument to the east, west, north, south, &c. but in every case I obtained the same sort of daily motion. The question, therefore, respecting the law of va-

riation of this instrument, still remains to be submitted to fixed principles, although there can be no longer any doubt that it is subject to a daily change.

On a curious anomaly observed between the daily variations in-doors and in the open air.

I have already mentioned that I was, at the commencement of my experiments, a good deal embarrassed and delayed by certain anomalies which I noticed between the daily changes of the needle made in the house and in the garden. These may be stated shortly as follows. That in certain positions of the needle towards the east and west, the daily motion, although it proceeded with the same determinate uniformity in both cases, yet it took place in different directions; passing in the one instance from the east, or west, towards the south, and in the other towards the north, at the same corresponding hours of the day, the motion in both instances being equally distinct, regular, and progressive.

After carefully examining every circumstance that might be supposed to be the cause of this singular change, I could only imagine three, that seemed in any way likely to account for it.

1st. Were the two magnets and the compass needle in the two cases in precisely the same relative situation? and if not might not the cause lie in this discrepancy?

2nd. The window of the room was to the northward; was it possible that the light, arriving at the needle in this direction, was the cause of the change?

3d. There was an iron stove in the room, could it be that this was subject to a periodic encrease and decrease of magnetic power?

In order to examine the first of these cases, I measured very carefully the distance, direction, &c. of the compass and magnets while in the garden, and placed them in precisely the same relative situation in the parlour; still the motion in the two cases was reversed.

To examine the second, it occurred to me that if the direction of the motion depended upon that of the light, the needle ought to be wholly stationary in the dark, or when excluded from the solar rays. I therefore kept my room shut for two days, and only examined the needle by the light of a wax taper; but although there was certainly less motion on those days than usual, yet I could come to no satisfactory conclusion; but I still think that farther observations will show that the solar light,* and not the solar heat, is the principal operative agent in producing the daily variation. It remained, however, to examine the third query, which I attempted to do as follows. Having placed the compass in its former situation in the garden, I fixed on one side of it a ten inch howitzer shell, in the same direction with respect to the compass as the stove had in the parlour, and at such a distance that it might produce a sensible deviation in the needle, and which I afterwards adjusted to zero by a slight change in the position of the magnets, thus placing the needle, as I imagined, under similar circumstances in both cases, with respect to local attraction; but, notwithstanding I did in this way actually produce an alteration in the daily mo-

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* I am sorry I have not the necessary apparatus for repeating Morichini's experiment on the violet ray; but I would suggest to those who have, that the finest test to which this experiment could be submitted, would be to make use of a needle neutralized as above described, by which the magnetic property of the ray, if it possessed any, could not fail of showing itself.

tion, changing its maximum from eleven o'clock in the morning to about four o'clock in the afternoon, yet the direction of the motion was the reverse of what it was constantly found to be in-doors; the cause therefore of this perplexing anomaly still remains to be discovered.

It is proper to observe that Mr. Christie, having made some of his observations in-doors, and some in his garden, on two compasses at the same time, found the same reversion of motion in the two cases. His house is a mile distant from mine; he has no stove in the room in which the in-door experiments were made; and the only resemblance of situation is, that his window, like mine, is towards the north. It should be farther added, that this confirmation of the singular anomaly in question did not arise from his simply repeating my experiment, but grew naturally out of the particular mode he had adopted to prosecute the enquiry; our experiments, with the exception of the first suggestion, are independent, and therefore, where they both lead to the same result, they may be considered as confirming the accuracy of each; and where there is any difference, they will at least point out those circumstances which require farther investigation.

P. S. The experiments to which I have alluded in page 337, made since this article was written, seem to indicate that this anomaly, as well as the circumstance there mentioned, may be occasioned by the daily varying intensity of the opposing magnets.

XXIV. *On the Diurnal Deviations of the Horizontal Needle when under the influence of Magnets.* *By* Samuel Hunter Christie, *Esq. M. A. Fellow of the Cambridge Philosophical Society: of the Royal Military Academy. Communicated by Sir* Humphry Davy, *Bart. Pres. R. S.*.

Read June 19, 1823.

Having been for a considerable time engaged in investigating different magnetical phænomena, a suggestion of Mr. Barlow's, that the daily variation of the needle might be rendered more sensible by diminishing the directive force by means of a magnet, was received by me with much interest. He stated to me, that he proposed so to reduce the terrestrial force, that instead of the daily variation being only ten or twelve minutes, it should amount to three or four degrees, or more if necessary. In consequence of this I offered to make simultaneous observations, simply for the purpose of comparison; but having been led to prosecute the inquiry farther than I at first intended, I think that the observations which I have made, with much care, may not be deemed unworthy the attention of the Royal Society.

In making these observations, I adopted an arrangement different from that which Mr. Barlow informed me he proposed making use of. Instead of one magnet applied towards the end of the needle, and in the same horizontal plane with it, it appeared to me that a more equable distribution of the forces acting on the needle would be obtained, if I substituted

two, and still more so if these were placed in the line of the dip. According to the manner in which I have for a long time viewed the nature of the forces which give direction to the horizontal needle, and their disturbance by other forces, it appeared to me that, by applying two magnets to the needle in the line which it would take if freely suspended by its centre of gravity, but having their poles in the reverse position to those of the needle, one above and the other below its centre, a portion of the forces acting upon the horizontal needle in the line of these magnets, or of the dip, would be destroyed; and it would therefore still be acted upon by forces in the same direction as before, but of less intensity: whereas by even applying the poles of two magnets to the corresponding poles of the needle, and in the same plane with them, the horizontal directive force of the needle would be diminished, by increasing the angle which the resultant of the terrestrial forces, and those of the magnet made with the horizon; and which would be nearly equivalent to increasing the angle of the dip. This, however, will not be quite correct when the length of the needle bears a sensible ratio to the distances of the magnets; but, when this adjustment occurred to me, I was not aware that the magnets must be brought so near to the needle as I afterwards found necessary. Notwithstanding this, as I considered the arrangement to be a good one when the observations were to be made with the needle in the meridian, I adjusted, in the line of the dip, two powerful bar magnets, each twelve inches long, .95 inch wide, and .375 inch thick, to an instrument with which I had made a variety of magnetical experiments; one above the needle with its north pole towards the centre, to destroy a portion of the

force acting upon the needle from the centre upwards towards the south; the other below the needle with its south pole towards the centre, to destroy a corresponding portion of the force from the centre downwards towards the north: that is, both the magnets having their north poles downwards. To prevent any ambiguity, I must here state, that, by the *south pole* of a magnet, I understand always the end which, when the magnet is freely suspended, points towards the north pole of the earth; so that the *north end* is the *south pole*, and the *south end* the *north pole* of a magnetic needle. By diminishing the distances of the magnets from the centre, I diminished the directive force of the needle until I destroyed it, after which, by diminishing the distances, I increased the force by which the north end was directed towards the south.

Previously to giving any of the observations, to avoid unnecessary repetition, I will here state that, whenever the direction of the needle, or its deviation, or the direction of the forces urging it, is mentioned, reference is always made to the end of the needle which, when undisturbed, points towards the north, that is its south pole, unless the contrary is expressed. To compare more readily with each other, the directions of the deviations when the needle is held in different positions by means of the magnets, whenever the direction itself of the north end of the needle is not given, I consider the deviations which take place in the direction of the westerly, or principal daily variation to be plus, and those in a contrary direction minus: that is, in stating the deviations from any point, considered as zero, those which take place, from that point, in the direction of the sun's apparent

daily motion are considered minus, and those in a contrary direction plus, whatever may be the position of the needle.

The first observations which I made, being principally with a view of ascertaining how my apparatus would answer, and likewise the general characters of the deviations, were necessarily somewhat imperfect; but as they show the great extent to which the deviation may be rendered sensible, and first indicated to me distinctly, what I afterwards found so decidedly marked, the morning easterly deviation, I will not omit them. The needle which I made use of is a very light one, about $\frac{1}{10}$ of an inch broad close to where the agate is centered, and diminishing from there to very fine points at the ends: its length is six inches. The rim within the compass-box is very accurately divided into degrees, and thirds of a degree; so that, from practice, I can easily read off the directions of the needle to every two minutes. It may at first sight appear superfluous to mention any particulars respecting the room in which the observations were made, but as both Mr. Barlow and myself found some anomalies in the directions of the needle, when corresponding observations were made in different situations, and as these anomalies may perhaps be attributable to such local circumstances, I shall always state them as nearly as possible. The observations contained in the following table, were made in a room with a single window, facing E 40° S, magnetic, and having an iron grate in it, the situation of which was S 15° E, distant six feet from the needle; but care was taken that no iron should be moved in the room, nor any small articles of iron brought into the immediate neighbourhood of the needle.

In the first column of the table, the hours are marked at

which the several deviations from the magnetic meridian, in the second and following columns, were observed, and the day on which the observations were made is indicated above.

Observations of the Deviations of a Magnetic Needle having its directive power diminished by the action of two bar Magnets placed in the line of the Dip.

March	26	27	28	29	30	31
h. m.	° ′	° ′	° ′		° ′	° ′
7 00	——	−2 10	——	The magnets were removed farther from the centre, to render the directive force greater.	——	−0 40
8 00	——	——	−4 04		−1 06	——
9 00	——	−4 40	−2 30		−0 10	——
11 00	——	——	+2 12		+1 50	——
Noon.	——	+6 16	+8 20		+2 32	——
1 00	+ 9 20	——	+9 10		+3 10	+4 30
1 30	+10 40	+9 20	+8 10		——	——
2 00	+ 7 40	——	+7 26		+2 30	——
4 00	+ 4 34	——	+4 40		+1 06	——
6 00	+ 2 32	0 00	+3 30		+0 10	——
10 00	+ 0 04	——	——		+0 10	——

The deviation towards the east before 8 o'clock in the morning, and the greatest westerly deviation about 1 o'clock afternoon, are here very manifest.

As it was by the diminution of the directive force that the daily variation was rendered thus sensible, the needle possessed so little in the first three days observations, that there was a considerable degree of indecision about the point at which it settled; and although on the 29th of March I increased the directive power by placing the magnets at a greater distance, at the same time rendering that part of the apparatus which supported them more secure, I was by no means satisfied with the manner in which the needle came home. In consequence of this I made a new needle from part of a clock spring, which I considered to be excellent

steel. The form of the needle was that of two segments of circles joined by their chords, the sum of the versed sines, or the greatest breadth, namely, where the agate was centered, being .425 inch, and the chord 6 inches: the extremities were hardened and very accurately terminated in very sharp points. This I found to have much greater directive force than the other; making, when not under the influence of the magnets, twenty-three vibrations in 60 seconds, whereas the other made only eighteen vibrations in the same time; so that the directive force of the new needle was to that of the other as 1.63 to 1. I had also made other needles, of the same steel, of greater breadth in the middle, but found that this had considerably the greatest directive power. With this needle I made most of the subsequent observations.

Having placed the apparatus in a room having two windows facing W 40° N, and an iron stove at the distance of nine feet from the needle in the direction W 20° S, it was fixed so that zero of the arc in the compass-box corresponded with the magnetic north. On the 4th of April, at $10^h\ 35^m$ P. M. I adjusted the magnets, which I had before used, in the line of the dip, as I have already described, so that the needle, when under their influence, pointed to zero, and made five vibrations in forty seconds. As previously to applying the magnets the needle had made thirty vibrations in seventy-eight seconds, the directive force was diminished by them nearly in the ratio of 1 to .106. I here assume that the forces will be as the squares of the number of vibrations in the same time, which is not very accurate, since, when the directive force was much diminished, I was under the necessity of commencing the vibrations at 60° from zero, in order to obtain a sufficient number to estimate, at all correctly, their duration.

At 11^h 45^m P. M. the north end of the needle had settled at N 0° 04′ E; and on the following day I observed as follows, noting the direction which the needle had assumed between the observations, and likewise the point where it settled, after it had been agitated by gently tapping the compass-box.

5th April.	Direction of the N. end.	
Morning.	When first Observed.	After agitating the Needle.
h. m.	° ′	° ′
7 00	−0 04	−0 12
7 35	−0 46	−0 54
8 10	−0 30	−0 56
8 35	−0 50	−1 00
9 15	−1 16	−0 46
10 00	−0 20	−0 20
10 35	−0 10	0 00
11 05	−0 06	+0 06
0 05	+0 32	+0 42
0 30	+1 00	+0 58
1 00	+1 00	+0 58
1 30	+1 00	+0 56
2 00	+1 02	+0 58
4 15	+0 26	0 00
8 45	0 00	0 00

Here the times of the greatest easterly deviation in the morning, and of the westerly in the afternoon, correspond with the preceding observations, but the easterly appears much greater, compared with the westerly, than I had before observed. I had intended to continue observing for several days with the present adjustment of the needle, but being still not quite satisfied that the needle moved freely on the point of suspension, I carefully polished the inside of the agate cap, and also the point on which it rested, preparatory to the observations of the next day. As I found, after I had done this, that the needle settled with great precision, in order to obtain rather larger arcs of deviation, I adjusted the magnets

so that the needle made ten vibrations in one-hundred seconds, and consequently the directive force was still farther reduced; its ratio to the whole undiminished force being now .068 to 1. With this adjustment I observed very carefully the direction of the needle, nearly every hour, except during the night, from the evening of the 5th of April, till that of the 12th of April. On the 5th of April, 9^h 50^m P. M. the needle pointed N 0° 04′ W, and at 11^h 45^m it pointed N 0° 10′ E, but as it appeared afterwards to be most stationary in the evening at about N 0° 20′ E, that point is assumed as zero in the following table.

Table of the Diurnal Deviations of a Magnetic Needle whose directive force was diminished in the ratio of 1 *to* .068, *by the action of two bar Magnets placed in the line of the Dip, the N. end of the Needle pointing North.*

April	6	7	8	9	10	11	12	Means.
h. m.	° ′	° ′		° ′		° ′	° ′	° ′
6 00	———	—0 24	(4^h 15^m 0 00)	———	(5^h 15^m —0 22)	———	———	———
6 30	—1 00	—0 48	———	———	———	———	+0 04	—0 35
7 00	—1 00	———	———	—0 06	———	—0 24	—0 20	—0 28
7 30	—1 20	—1 16	———	—0 14	———	—0 08	—0 38	—0 43
8 00	—1 38	—1 00	—0 36	0 00	—1 02	———	—0 10	—0 44
8 30	—1 24	———	———	———	—1 16	———	—0 12	—0 57
9 00	—1 16	—0 44	—0 24	—0 10	—1 06	+1 04	+0 28	—0 18
10 00	—0 28	—0 04	+0 32	+0 16	———	+1 06	+0 46	+0 21
11 00	+0 28	+1 00	+1 40	+1 54	———	+1 40	———	+1 20
Noon.	+1 26	+2 00	+2 50	+2 30	+1 06	+2 40	+2 48	+2 26
0 30	———	+2 00	+2 50	———	+2 04	———	———	+2 18
1 00	+1 46	+2 1′2	+2 50	+2 32	———	+4 00	+3 36	+2 49
1 30	+1 40	+2 12	+2 44	———	+2 10	———	+3 56	+2 32
2 00	+1 40	+2 00	+2 18	+2 24	———	+4 20	+3 28	+2 41
2 30	+1 26	———	+2 10	———	+1 48	+4 14	———	+2 24
3 00	———	+1 16	+1 36	———	———	———	———	+1 26
4 00	+1 08	+0 52	+1 14	+1 06	+0 56	+3 08	+2 04	+1 13
5 00	+0 18	+0 28	+0 44	—0 04	+0 42	+2 04	———	+0 42
6 00	+0 02	+0 14	+0 34	—0 04	+0 28	+1 30	+0 16	+0 26
7 00	—0 04	0 00	+0 16	+0 12	+0 34	+0 54	+0 24	+0 20
8 00	———	—0 06	0 00	—0 06	+0 40	+0 20	+0 26	+0 12
9 00	0 00	+0 08	———	———	———	+0 38	+0 14	+0 15
10 00	———	+0 12	+0 04	—0 58	+0 28	(—1 40)!	———	—0 03
11 00	0 00	+0 08	—0 10	—0 16	0 00	—0 06	———	—0 04

As the corresponding observations could not always be taken at precisely the same hour, they are put down to the nearest half hour: they however were generally taken within five or ten minutes of the times specified. In estimating the times of the maxima deviations, I take those at which the observations were actually made: and for the time of zero, I take a proportional time between the nearest observations. This notice will apply to the other tables of the deviations.

From these observations I deduce the following table.

Table of the greatest Easterly and Westerly Deviations, their times, the times of Zero, or no deviation, and the total daily changes in the direction of a Needle, whose directive force was diminished in the ratio of 1 *to* .068, *by the action of two bar Magnets placed in the line of the Dip: the N. end of the Needle pointing North.*

April.	Max. E. or minus.		Time of Zero. Morning.	Max. W. or plus.		Time of Zero. Afternoon.	Total change.
	Value.	Time.		Value.	Time.		
	° ′	h. m.	h. m.	° ′	h. m.	h. m.	° ′
6	—1 38	8 00	10 30	+1 46	1 45	6 30	3 24
7	—1 16	7 30	10 04	+2 12	1 15	7 00	3 28
8	—0 36	8 00	9 15	+2 50	0 45	8 00	3 26
9	—0 14	7 35	9 20	+2 32	0 50	5 15	2 46
10	—1 16	8 30	10 22	+2 10	1 30	——	3 26
11	—0 24	7 10	7 50	+4 20	2 00	6 25	4 44
12	—0 38	7 30	8 42	+3 56	1 40	7 00	4 34
Mean	—0 52	7 45	9 26	2 49	1 24	6 40	3 41

During the time that I was making these observations, and likewise all the subsequent ones, I noted the general characters of the weather, but as I neglected to observe the states of the thermometer and barometer, such loose observations would, to others, be of little use, and I therefore omit them, although at the time they served to convince me that the

state of the weather had considerable influence on the nature and extent of the changes. On several occasions I certainly noticed that a cold cloudy morning, with the wind easterly, appeared to be very unfavourable to the increase of the easterly deviation, but I would not venture to draw any general conclusions from such observations. After having made these observations, I still farther diminished the directive power of the needle, by bringing the magnets nearer to the centre. The needle now made five vibrations in seventy seconds, so that the directive force was diminished in the ratio of 1 to .034, or was half what it was in the preceding case.

Table of the Diurnal Deviations of a Magnetic Needle, whose directive force was diminished in the ratio of 1 *to* .034, *by the action of two bar Magnets placed in the line of the Dip : the N. end of the Needle pointing North.*

April	15	16	17	18	19	Mean.
h. m.	° ′	° ′	° ′	° ′	° ′	° ′
7 0	—	−0 40	—	−5 00	−3 40	−3 07
7 30	−2 20	−0 20	−3 10	−5 40	—	−2 53
8 0	—	—	−2 40	—	−3 02	−2 51
9 0	−3 00	+1 44	−1 10	−3 00	+1 40	−0 45
10 0	+0 58	+5 30	—	+2 10	+1 20	+2 30
11 0	+3 22	+5 58	—	+4 56	—	+4 45
Noon.	—	+7 00	+4 34	+6 20	—	+5 58
1 0	+6 40	—	—	—	+8 16	+7 28
1 30	—	+8 14	+4 20	+4 56	—	+5 50
2 0	+5 44	+7 00	—	—	+5 50	+6 11
4 0	—	+4 40	+2 50	+1 20	+1 54	+2 41
6 0	+0 28	—	−2 12	+0 50	−0 18	−0 18
8 0	—	—	−2 34	+1 44	+2 10	+0 27
10 0	—	—	—	+3 00	—	—
11 0	+2 06	+1 10	−5 30	—	—	−0 45

From these I derive the following

Table of the greatest Easterly and Westerly Deviations, their times, the times of Zero, and the total daily changes in the direction of a Magnetic Needle, whose directive force was diminished in the ratio of 1 to .034, by the action of two bar Magnets placed in the line of the Dip: the N. end of the Needle pointing North.

April.	Max. E. or minus.		Time of Zero.	Max. W. or plus.		Time of Zero.	Total change.
	Value.	Time.	Morning.	Value.	Time.	Afternoon.	
	° ′	h. m.	h. m.	° ′	h. m.	h. m.	° ′
15	—3 00	9 00	9 44	+6 40	0 55	6 10	9 40
16	—0 40	7 00	7 50	+8 14	1 30	——	8 54
17	—3 10	7 40	10 30	+4 34	0 05	4 40	7 44
18	—5 40	7 30	9 36	+6 30	0 15	——	12 10
19	—3 40	7 10	9 00	+9 24	0 50	6 15	13 04
Mean	—3 14	7 36	9 20	+7 04	0 43	5 02	10 18

Comparing these mean results with those in the preceding table of maxima, we find the times in general earlier: the observations are however much too limited to point out a very correct mean in either case.

After having made these observations I entirely removed the magnets, and found that the needle made thirty-eight vibrations in one hundred seconds, so that its directive power continued nearly the same as at the commencement.

For the purpose of exhibiting at one view the facts pointed out by these observations, I have, in the following diagram, [Pl. XXIV.] placed the deviations as ordinates to a curve, the distances between them representing the intervals of time. The axis of the curve corresponding to zero, the westerly, or plus deviations, are measured to the right, and the easterly, or minus deviations, to the left. The days on which the observations were made are placed over the respective curves, and

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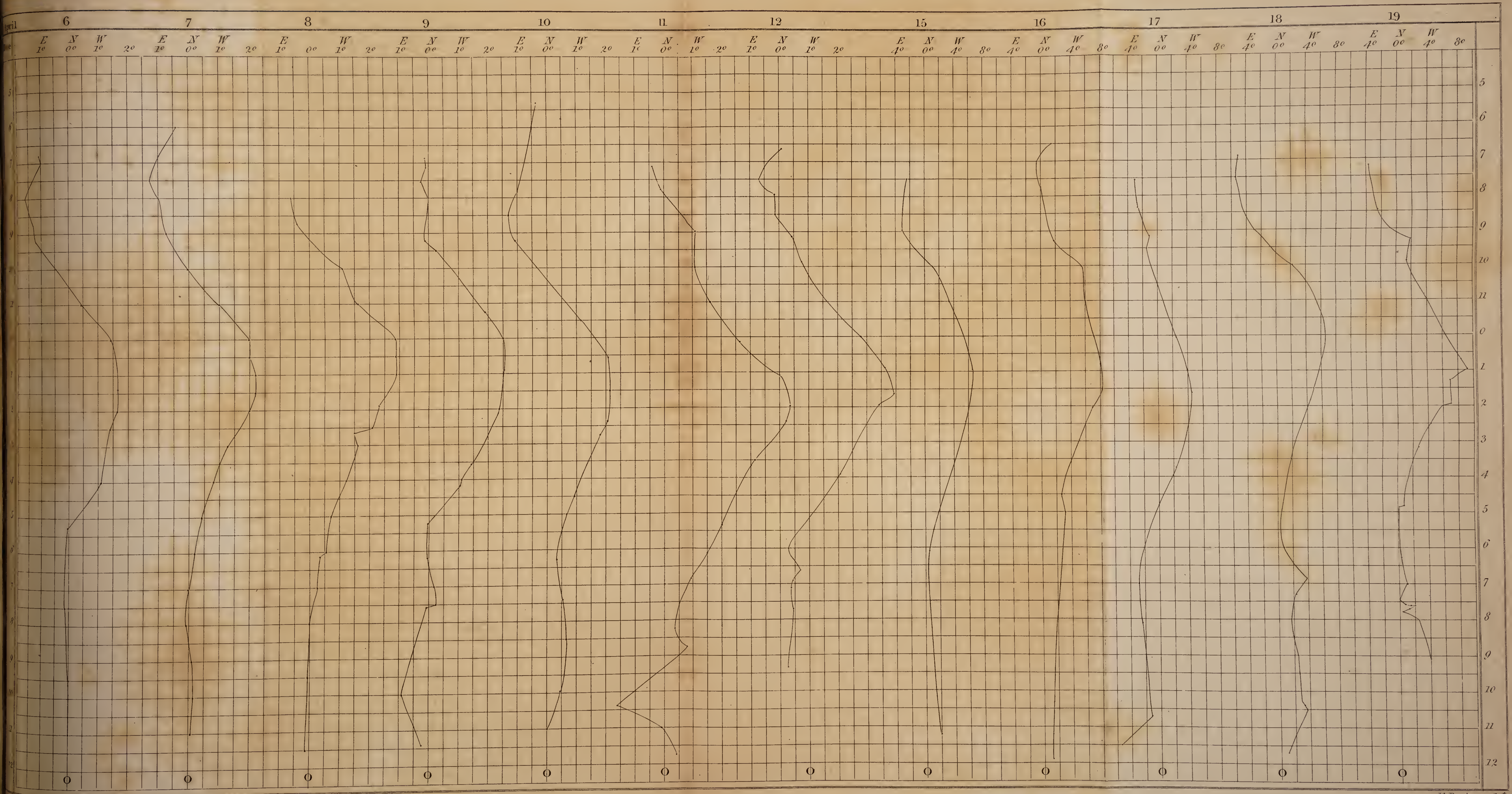
Curves of the diurnal deviations of a magnetic needle whose directive power was diminished by the action of two bar magnets placed in the line of the dip: the N. end of the needle pointing north.



J. Basire sculp.

the hours of the day are marked in the lines of the several ordinates. The points determined by actual observation are marked with a dot, and are connected by a line, as nearly as I could judge, in the direction which the curve would have taken had more points been so fixed. When the observations were made, it had not occurred to me to represent them in this manner, or they would have been more multiplied for the purpose: this defect I remedied when I observed the deviations with the north end of the needle to the south.

Many anomalies are here apparent, but I cannot attribute any of them to particular local causes, such as the moving of iron in or near the room. The door of the room was locked, so that nothing could be moved in it without my being aware of it; and whenever I found any peculiarity in the deviation, I minutely examined the adjacent rooms to ascertain whether it could have been produced by any article of iron having been introduced or moved in them. This caution was perhaps unnecessary, since I had found that transferring the fire irons from one part of the room to another, at nearly the same distance from the needle, did not produce any very sensible effect. I have before omitted to state that there was no fire in the room: had there been one, the difference in the temperature of the iron might have produced a slight effect.

That several of these irregularities were caused by the electric state of the clouds I have no doubt; and for the purpose of showing how distinctly the effects are marked by a needle under such circumstances, I subjoin the observations made on the 19th, at times when, although there was no thunder, there were unequivocal signs of the electric state of the atmosphere.

16th April. Morning cold and cloudy; wind N.W. strong.

Time.	Direction of Needle.	
h. m.	° ′	
0 40	8 16 W	
0 50	9 24 W	Showers of hail and heavy rain.
1 15	7 08 W	A heavy cloud and hail shower in W: cloud apparently highly electric.
1 50	7 00 W	Cloud passed to S; sun bright.
2 00	5 40 to 5 50 W	At several observations.
4 45	1 00 W	Gradually decreased to 0° 10′ W. A heavy shower of hail: the cloud from W, though S to E and in zenith.
6 15	0 18 E	
7 00	0 56 W	A heavy cloud from N to NW; greatest height that of the equator of the dipping needle.
7 05	0 16 W	
7 25	0 10 W	Cloud passed to S.
7 32		Changed to 0° 50′ W, 1° 40′ W, 2° 00′ W, then gradually went back to 1° 10′ W, 0° 50′ W, 0° 40′ W, settling at 0° 30′ W.
7 43	0 30 W	

In some of my first observations, I had noticed that the directions of the needle were different, according as I stood to the east or to the west of the needle; and although I did not then attribute this effect to the right cause, namely, a very minute change in the level of the instrument, I always took particular care, by placing myself directly in front of the instrument when I observed, that this should not affect the observations: in those which I am about to describe, the possibility of any effect of this kind was avoided by the instrument standing on a stone floor, which was laid on the ground itself.

Having ascertained the general character of the deviations when the north end of the needle pointed towards the north, my next observations were with a view of comparing with it, that of the deviations when the north end pointed towards the south. For this purpose the magnets were brought

nearer to the needle, their nearest ends being at the distance of 9.6 inches from the centre; and they were carefully adjusted in the magnetic axis, or line of the dip, so that the north end of the needle pointed very accurately to the south, or 180° of the instrument. In this position I vibrated the needle, as before, and found that it made ten vibrations in one hundred and five seconds. From this it appears that the force by which the needle was now directed towards the south, was to the undiminished terrestrial force towards the north, as .0625 to 1; and was therefore rather less than the force towards the north in the observations from the 6th to the 12th of April. The instrument was placed near the outer wall in a room having a single window facing N 40° E; the only iron in the room being a large lock to the door and the weights to the windows, which were always in the same position when the observations were made.

The observations were begun on the 21st of April, and continued to the 27th, when they were unavoidably interrupted. Whilst making them, after the first two days, I perceived a gradual increase in the deviation easterly, but as I could only attribute this to a small change taking place by degrees in some part of the instrument, any correction of it must have been in a great measure arbitrary. Had I reduced the observations each day, by considering the morning and evening nearly stationary points as zero, it would have had the effect of separating the evening observations of one day from the morning ones of the next: I therefore preferred giving the observations as they were made, although on the last day the accumulated increase amounted to 2°.

Table of the Diurnal Deviations of a Magnetic Needle having the North end held in equilibrio, at South by two bar Magnets placed in the line of the Dip.

April	20	21	22	23	24	25	26	27	Mean
h. m.	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
5 00	—	—	—	—	+ 2 26	—	+ 2 38	+ 2 48	—
5 30	—	—	—	—	+ 2 26	—	+ 2 50	+ 3 02	—
6 00	—	—	+ 1 20	—	+ 2 42	—	+ 2 58	+ 3 06	—
7 00	+ 1 22	+ 2 12	+ 1 44	+ 1 46	+ 2 50	+ 2 48	+ 3 30	+ 3 30	+ 2 19
7 30	—	+ 2 30	+ 1 56	+ 1 52	+ 3 02	+ 2 48	+ 3 34	+ 3 48	+ 2 37
8 00	+ 1 28	—	+ 2 02	+ 2 08	+ 3 02	+ 2 44	+ 3 36	+ 3 56	+ 2 30
8 30	—	—	+ 2 04	+ 1 56	—	—	+ 3 36	+ 4 00	—
9 00	+ 1 08	+ 2 18	+ 1 24	+ 1 40	+ 2 38	+ 2 36	+ 3 18	+ 3 54	+ 2 09
10 00	+ 0 08	+ 1 24	+ 0 56	+ 1 20	+ 1 34	+ 1 42	+ 2 36	+ 2 58	+ 1 23
11 00	— 0 22	+ 0 02	— 0 02	+ 0 44	—	+ 0 36	+ 1 28	+ 1 42	+ 0 24
Noon	— 0 56	— 1 06	— 0 40	+ 0 26	+ 0 26	+ 0 22	+ 0 44	+ 0 48	— 0 06
0 30	— 1 12	— 1 10	—	—	—	+ 0 16	+ 0 34	+ 0 48	—
1 00	— 1 24	—	— 0 40	+ 0 12	+ 0 06	+ 0 10	+ 0 22	+ 0 44	— 0 12
1 30	— 1 20	— 1 40	—	—	— 0 06	—	+ 0 26	—	—
2 00	— 1 00	—	— 1 00	+ 0 10	— 0 02	— 0 14	+ 0 52	+ 1 00	— 0 12
3 00	— 0 34	— 0 34	— 0 44	+ 0 28	+ 0 06	+ 0 44	+ 1 12	Observations discontinued.	+ 0 05
4 00	—	— 0 20	+ 0 20	+ 0 06	+ 0 22	+ 1 20	+ 2 40		+ 0 45
5 00	— 0 12	+ 0 08	— 0 14	+ 1 00	+ 0 42	+ 1 52	+ 2 28		+ 0 49
6 00	— 0 14	— 0 28	+ 0 24	+ 0 40	+ 1 20	+ 2 06	+ 3 02		+ 0 59
7 00	0 00	+ 0 22	+ 0 02	+ 1 36	+ 1 18	+ 2 06	+ 3 10		+ 1 13
8 00	—	+ 0 14	+ 0 18	+ 1 42	+ 1 12	+ 2 00	+ 2 50		+ 1 23
9 00	0 00	0 00	+ 0 16	+ 1 40	+ 1 46	+ 1 54	+ 2 52		+ 1 13
10 00	+ 0 04	— 0 06	+ 0 26	+ 1 40	+ 1 56	+ 2 02	+ 2 40		+ 1 15
11 00	—	0 00	+ 0 32	+ 1 38	+ 1 54	+ 2 00	+ 2 40		+ 1 27

As there are not corresponding observations on all the days, and there appears to be a considerable change in the point which should be considered as zero, a very correct mean of all the observations cannot be taken; but I have adopted that which embraces the greatest number. I exclude the observations of the 27th, as not carried through the whole day, and of the others, I take the means of those at the several hours on which there were observations for at least six days.

I have stated that the deviations are those actually observed from 180° or the south point, and the mean deviations will be from the same point; but to have the mean deviations from the most stationary point, or that which should be considered as zero, it is necessary to ascertain, as correctly as possible, the mean time of the needle being stationary, and its mean direction at that time. Now we have the observations at every hour from three till eleven on every day but the 20th and 27th, and taking the means of these six days, we shall have the deviations as under.

at $3^h 0^m$ P.M.	+ 0° 12′
4 0 - -	+ 0 45
5 0 - -	+ 0 59
6 0 - -	+ 1 11
7 0 - -	+ 1 25
8 0 - -	+ 1 23
9 0 - -	+ 1 25
10 0 - -	+ 1 26
11 0 - -	+ 1 27

So that from seven o'clock the mean change in direction is extremely small, and the mean situation of the stationary point is + 1° 25′. Considering this point as zero, we shall have the mean deviations at the different hours as follow:

Time.	Deviation.
h. m.	° ′
7 0	+ 0 54
7 30	+ 1 12
8 0	+ 1 05
9 0	+ 0 44
10 0	— 0 02
11 0	— 1 01
Noon.	— 1 31
1 0	— 1 37
2 0	— 1 37
3 0	— 1 13
4 0	— 0 40
5 0	— 0 26
6 0	— 0 13
7 0	0 00

Taking the whole of the observations, the direction at nine o'clock appears to be that from which the evening observations vary least: considering then the direction at nine o'clock as the zero of each day, we obtain the following

Table of the greatest Easterly and Westerly Deviations, their times, the times of Zero, and the total daily changes in the direction of a Magnetic Needle having its North end held in equilibrio, at South by two bar Magnets placed in the line of the Dip.

April	Max. E. or Plus.		Zero.		Max. W. or Minus.		Total change.
	Value.	Time.	Value.	Time.	Value.	Time.	
	° ′	h. m.	° ′	h. m.	° ′	h. m.	° ′
20	+1 28	7 55	0 00	10 18	−1 24	0 48	2 52
21	+2 30	7 40	0 00	11 00	−1 40	1 35	4 10
22	+1 48	8 30	0 16	10 40	−1 16	2 05	3 04
23	+0 34	8 08	1 40	9 00	−1 30	1 30	2 04
24	+1 16	7 40	1 46	9 57	−1 52	1 45	3 08
25	+0 54	7 15	1 54	9 47	−2 08	2 00	3 02
26	+0 44	8 15	2 52	9 30	−2 32	1 15	3 16
27	+1 08	8 30	2 52	10 06	−2 08	1 00	3 16
Mean	+1 18	7 59	1 25	10 2	−1 49	1 30	3 6

The times of the corresponding maxima in the several days agree with each other, and likewise with the observations at north, as nearly as we could expect, but those of zero are more at variance. This however is accounted for by considering that this point has been, in some measure, assumed arbitrarily, in consequence of the gradual change in the direction. The mean total changes here and at north, likewise agree very nearly with each other, taking into the account the difference of the intensities in the two cases; and particularly if we take the means of those observations which

agree most nearly with one-another at the same point: they are,

	At South.	At North.
	2 52	—
	3 4	—
	3 8	3 24
	3 2	3 28
	3 16	3 26
	3 16	3 26
Means	3 6	3 26

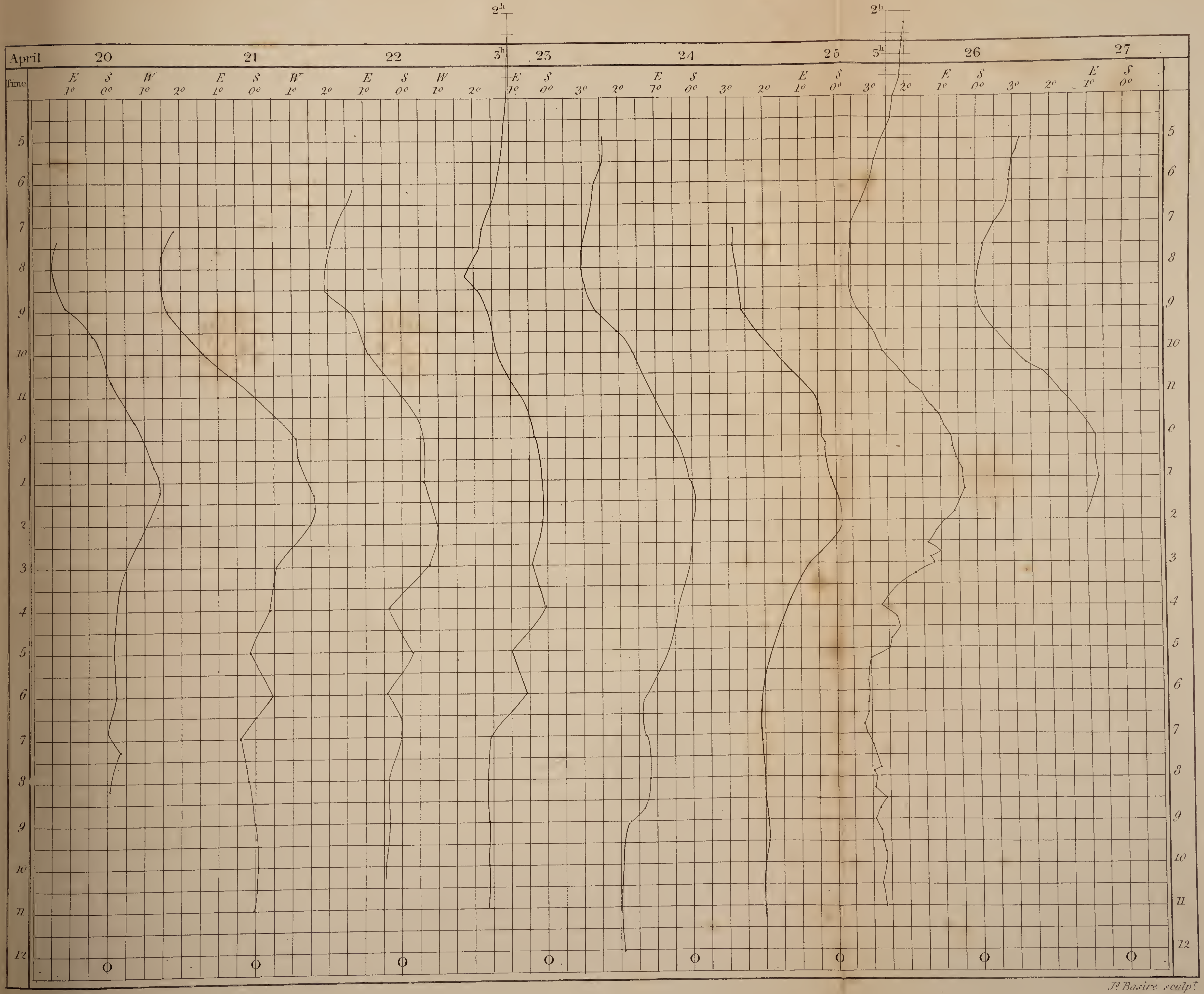
and the proportion of the intensities 100^2 to 105^2, would give 3° 6′ and 3° 25′.

Being desirous of observing the changes from the earliest time in the morning, I commenced the observations on the 26th, at 2^h 15^m A. M. and continued them throughout the day at intervals of half an hour, and sometimes less, until 11^h P. M. As they exhibit very minutely all these changes, and point out clearly the early period at which they commence, I here subjoin them at length. In the second columns are the directions which the needle had when undisturbed at the times specified in the first columns, and in the third are the points at which it settled, at the times marked in the fourth columns, after being very gently vibrated. Where there are two directions corresponding to any time, it indicates that, when observed, the needle kept vibrating between them. I have added the morning observations of the 27th till past the easterly maximum, that the evening deviations might again be connected with those of the following morning in one view. At 11^h 15^m P. M. April 25th, the needle pointed + 2 00.

Table of the Deviations of a Magnetic Needle having the North end held in equilibrio, at South by two bar Magnets placed in the line of the Dip, on the 26th April, 1823.

Before Noon.				After Noon.							
Observations				Observations				Observations			
Commenced, undisturbed.		Concluded, after vibration.		Commenced, undisturbed.		Concluded, after vibration.		Commenced, undisturbed.		Concluded, after vibration.	
Time.	Direction.	Direction.	Time.	Time.	Direction.	Direction.	Time.	Time.	Direction.	Direction.	Time.
h. m.	° ′	° ′	h. m.	h. m.	° ′	° ′	h. m.	h. m.	° ′	° ′	h. m.
2 15	+2 00	+2 00	2 17	0 12	+0 40	+0 38	0 15	6 30	+3 04	+3 06	6 31
2 32	2 00	2 04	2 35	0 28	0 34	0 32	0 30	6 44	3 10	3 12	6 45
2 57	2 03	2 08	3 00	0 43	0 28	0 20	0 45	6 59	3 10	3 10	7 00
3 28	2 08	2 10	3 30	0 58	0 22	0 22	1 00	7 14	3 08	2 58	7 15
3 57	2 10	2 18	4 00	1 14	0 22	0 20	1 15	7 29	2 54	2 50	7 30
4 28	2 20	2 22	4 31	1 27	0 22	0 26	1 30	7 44	2 26 2 22	2 46	7 45
4 56	2 24	2 38	5 01	1 44	0 32	0 38	1 45	7 59	2 52	2 50	8 00
5 27	2 40	2 50	5 30	1 58	0 40	0 52	2 00	8 14	2 52	2 52	8 15
5 57	2 52	2 58	6 00	2 14	0 54	1 08	2 15	8 30	2 46	2 34	8 31
6 28	3 02	3 14	6 30	2 31	1 12	1 20	2 33	8 44	2 42	2 46	8 45
6 57	3 20	3 30	7 00	2 43	1 08	0 58	2 45	8 59	2 46	2 52	9 00
7 27	3 28	3 34	7 30	2 51	1 12	1 16 1 10	2 52	9 14	2 48	2 44	9 15
7 57	3 34	3 36	8 01	2 58	1 10	1 12	3 00	9 30	2 46	2 42	9 31
8 27	3 36	3 36	8 30	3 13	1 27	1 44	3 15	9 44	2 42	2 40	9 45
8 56	3 30	3 18	9 00	3 28	1 46	2 12	3 30	9 58	2 38	2 40	10 00
9 27	3 10	2 56 2 50	9 30	3 43	2 20	2 24 2 34	3 45	10 29	2 42	2 44	10 30
9 57	2 44	2 36	10 00	3 58	2 36	2 40	4 00	10 58	2 44	2 40	11 00
10 29	2 10	2 06	10 30	4 13	2 20	2 18 2 14	4 15	16 57	2 44	2 48	17 00
10 42	2 00	1 47	10 45	4 26	2 16	2 06 2 20	4 30	17 29	2 56	3 02	17 30
10 57	1 30	1 28 1 24	11 00	4 42	2 20	2 24	4 45	17 57	3 04	3 06	18 00
11 07	1 25	1 20	11 10	4 58	2 26	2 28	5 00	18 29	3 09	3 10	18 30
11 13	1 22	1 18 1 14	11 14 11 15	5 14	2 42	3 00	5 15	18 59	3 18	3 30	19 00
11 23	1 10	1 10 1 08	11 25	5 29	3 00	3 04	5 30	19 29	3 40	3 48	19 30
11 28	1 08	1 04	11 30	5 44	3 06	3 04	5 45	19 59	3 48	3 56	20 00
11 43	1 00	0 56	11 45	5 59	3 04	3 02	6 00	20 28	3 56	4 00	20 30
11 57	0 52	0 44	0 00	6 14	3 02	3 04	6 15	20 59	3 58	3 54	21 00

Curves of the diurnal deviations of a magnetic needle having the North end held in equilibrio at South, by two bar magnets placed in the line of the dip.



Js. Basire sculpt.

During this day and the following, until the observations were discontinued, I observed the barometer every hour, but as it was remarkably steady, (varying during the 26th only from 29.77 at 3^h A. M. gradually to 29.75 at 3^h P. M., and from that to 29.82 at 11^h P. M., and on the 27th from 29.92 at 5^h A. M., to 30.075 at 1^h P. M.,) change of atmospheric pressure could have little influence in the deviations which took place. I regret much that I did not observe, what I now consider of more importance, the temperature both of the instrument and of the atmosphere. There was continued rain nearly through the whole day, no sensible change taking place in the temperature of the air; and I am persuaded, from this circumstance and the situation of the place in which the observations were made, that no material change took place in that of the instrument.

The observations from the 20th to the 27th of April, are exhibited [Pl. XXV.] in the same manner as were those at north. In the present instance, the easterly deviations were in a contrary direction to what they were in the former case, but I have still placed them towards the left, that the similarity of the curves in the two cases may be the more evident.

The principal irregularities to be observed here take place in general from about three o'clock in the afternoon to seven o'clock; and we might almost be led to infer from this circumstance that there are two causes, in opposition to each other, producing the deviation, and that these being nearly equal at this time, alternately preponderate: but the observations are too limited to allow of our drawing such general inferences from them. We shall see, from observations in other positions of the needle, that irregularities very frequently take place during the afternoon.

In this position of the needle, the morning deviation is at first towards the east, and afterwards towards the west, the same as when the north end of the needle pointed north, the needle tending towards the same point in the two cases, but in contrary directions. This is what we might expect from a simple change in the direction of the terrestrial force, but the observations which I shall presently describe, clearly point out both a change of direction and of intensity.

Having ascertained the nature of the changes which take place with the north end of the needle towards the north, and also towards the south, I proceeded to make observations with the north end in other directions. For this purpose I had prepared another compass, in order that I might observe at different points at the same time. This compass was placed in my garden, that it might be completely out of the influence of the magnets adjusted to the other needle, and likewise that I might be able to notice, whether, as Mr. BARLOW had found, the deviations were in contrary directions in the two cases, when the north end had the same, or nearly the same, direction in both. The observations were not, in this point of view, very conclusive, because although the deviations differed in many respects, in some they agreed, and likewise because the magnets were not adjusted in the same manner in the two cases; those within doors being in the line of the dip, and those to the other needle being in the meridian, on the same horizontal table as the compass. The points at which the observations were made within doors were the north end at N 28° W and N 49° W; and with the other needle at N 55° W. After continuing to make these observations carefully for some days, it occurred to me that I

might so adjust the magnets, that I could make simultaneous observations on the same needle in three different positions, without altering the situations of the magnets, or of the centre of the needle. As I consider that these observations point to conclusions which cannot fairly be drawn from those made at several points, under different adjustments of the magnets, I shall omit giving those mentioned above.

Previously to giving the observations themselves, I shall describe the principle on which I adjusted the magnets, and state what I considered would be the effects of certain changes in their distances from the centre of the needle, and in the angle which their axes made with the meridian, describing likewise the experiments, which proved the correctness of these conclusions: this will enable me the better to draw inferences from the observations. It is very evident that if the magnets are exactly in the meridian, at the same distances from the centre of the needle, and in the same horizontal plane with it, whatever may be the directions of the resultants of all the forces acting on the needle in any particular position of its ends, if that be changed for one diametrically opposite, the directions of the resultants will also be changed into others directly opposite; so that for every position of *stable equilibrium*, where the resultants are in the directions from the centre towards the extremities of the needle, there will be a corresponding position of *instable equilibrium*, where the directions of the resultants are from the extremities of the needle towards its centre. Calling zero, the point of *stable equilibrium* for the north end, when the needle is free from every other action but terrestrial magnetism, 180° will be the point of *instable equilibrium.* If

the magnets are in the meridian, and their poles are placed towards those of the needle of the same name, zero will still continue the point of *stable equilibrium*, and 180° of *instable equilibrium*, while the terrestrial forces are greater than those of the magnets; but when the forces of the magnets are equal to the terrestrial forces, both these points become what may be termed points of indifferent equilibrium, since the needle is not retained in the one by forces acting from the centre towards the extremities, nor do any forces act from the extremities towards the centre in the other: the equilibrium, however, at zero still possesses one character of stability, a tendency to return to it when disturbed, whilst that at 180° has the character of instability, a tendency to recede from it, under the same circumstances. If the magnets be brought somewhat nearer, the forces which they exert on the ends of the needle will be greater than the terrestrial forces, and zero will therefore become a point of *instable*, and 180° of *stable equilibrium*. It is besides evident, that the excess of the forces of the magnets above the terrestrial forces may be such, that they will be in equilibrio between zero and 180°; so that there will be a point of *stable equilibrium* between zero and the west, and zero and the east. Corresponding and opposite to these there will be two points of *instable equilibrium*; one between 180° and the east; the other between 180° and the west. There would therefore now be six points of equilibrium, alternately *stable* and *instable*. As the magnets are made to approach the needle, the two easterly points of equilibrium will approach each other, and also the two westerly; and when they coincide at east and west, the four will become two points of indifferent equilibrium, the resultants of all the

forces being here nothing. The magnets having approached beyond this, there will be only two points of equilibrium, a point of *stability* at 180°, and of *instability* at zero. The same effects would manifestly be produced, if instead of the force of the magnets being increased by their approach towards the centre, the terrestrial forces were to decrease, whilst the magnets remained at the same distances: so that the receding of the easterly and westerly points of stable equilibrium from the north, would indicate a diminution, and their approach to it, an increase of the terrestrial magnetic intensity.

Let us now consider what would be the effect if the line in which the magnets are placed described a small angle from the meridian, the magnets remaining at the same distances from the centre; or, which is the same thing, if the direction of the terrestrial forces were changed without any change in their intensities. We will suppose that the line in which the magnets are placed moves through a small angle in the direction of the sun's motion, which is equivalent to supposing that the magnetic meridian describes a small angle in the contrary direction.

Taking first the case of there being only one point of *stable equilibrium*, it is evident that when it is at zero, it will move in a direction contrary to that of the sun's motion, and that it will move in the same direction as the sun, when it is at 180°.

When there are three points of *stable equilibrium*, that which is at 180° will still move in the direction of the sun's motion, and the opposite point of *instable equilibrium* will move in the same direction, that is, from its position at zero, towards the *east*. In consequence of the decreasing angle

which the repulsive force, acting on the needle at the *easterly* point of *stable equilibrium*, makes with the needle, its stability will be diminished, and this point will approach the northern point of instability, until the two coincide: and beyond this the one will be transferred to the *westerly* point of *stability*, and the other to the *easterly* point of *instability*. In the mean time the *westerly* point of *stable equilibrium*, in consequence of the repulsive force of the magnet acting at a greater angle, will move farther towards the *west*: the three points ultimately forming but one.

If the line of the axes of the magnets revolve in a direction contrary to that of the sun's motion, or which is the same thing, if the magnetic meridian describe an angle in the direction of the sun's motion, effects the reverse of these will evidently take place.

I had drawn these conclusions, and proposed applying them to the observations on the daily changes in different directions of the needle, when it occurred to me that it would, after all, be more satisfactory, to apply to these observations, the facts that might be actually observed, by making the distances of the magnets and the line of their axes undergo the changes I have specified. In consequence of this I made the following experiments. A strong table being prepared, with the legs firmly driven into the ground, at a distance from any building, the compass with which I had made the preceding observations, and which, for the sake of distinction, I shall call No. 1, was placed on a meridian line very carefully drawn. The magnets which I had always used with this needle, were then placed on this line, at equal distances from the centre of the

needle, having their poles directed towards those of the same name of the needle, and their axes coinciding with the meridian, so that the north end of the needle still pointed to zero. The magnets were then brought in towards the centre, their axes still coinciding with the magnetic meridian, until the distances of their nearest ends were each 16.95 inches, when the north end was in equilibrio at each of the three points,

N 25° 26′ W, N 20° 10′ E, S 2° 20′ W.

that is, the needle being led near each of these points by means of a small piece of iron wire, settled steadily at each, after agitation.

I now drew lines perpendicular to the meridian, from the ends of the magnets, and measured off distances of .05, .10, .15, .20, .25 of an inch on each side of the magnets at the ends nearest to the centre, and distances at the farther ends corresponding to them; so that the sides of the magnets coinciding with the respctive points, the north magnet towards the east and the south magnet towards the west, or vice versa, the line of their axes had described angles from the meridian, about the centre, the tangents of which were $\frac{5}{1695}$, $\frac{10}{1695}$, $\frac{15}{1695}$, &c. respectively, or very nearly angles of 10′, 20′, 30′, &c.

In the following table, the angles which the north end of the line of the axes of the magnets made with the meridian are placed in the first column, and the points of equilibrium corresponding to these situations of that line are placed opposite to them in the following columns. When *not stable* is placed in the column, it is to be understood that the needle rests near the point above, but, on being in the least agitated, moves to the point which is found to continue stable throughout. When *no point* is placed, it is to be understood that the

needle does not rest at any point corresponding to those which had before been observed.

Table of the Points of Equilibrium corresponding to the several angles between the meridian and the axis of the Magnets, the distances of the Magnets from the centre of the Needle remaining the same. 11th May, from 6ʰ A. M. to 9ʰ A. M. compass No. 1.

North end of the axis towards East.				North end of the axis towards West.			
Angle.	Points of Equilibrium.			Angle	Points of Equilibrium.		
N. 0° 0′ E.	N. 25° 26′ W.	N. 20° 10′ E.	S. 2° 20′ W.	N. 0° 0′ W.	N. 25° 26′ W.	N. 20° 10′ E.	S. 2° 20′ W.
0 10	27 58	not stable	10 6	0 10	21 16	21 0	S. 4 12 E.
0 20	29 28	not stable	17 48	0 20	15 10	25 32	16 0*
0 30	30 2	no point	no point.	0 30	no point	27 36	no point
0 40	31 40	no point	no point.	0 40	no point	29 56	no point
				0 50	no point	31 34	no point

From the first part of this table it appears, that, if the north end of the axis of the magnets described an angle from the meridian towards the east, without any change in the intensities of the forces which the magnets exerted on the needle, or, which amounts to the same thing, if the direction of the force urging the north end of the needle deviated towards the *west*, without any change in its intensity, the *westerly* point of equilibrium would move towards the *west*: and also the *easterly* and *southerly* points of equilibrium would move towards the *west*, until they were lost in the *westerly* point: so that, the directions of the deviations of the *easterly* and *westerly* points of equilibrium would be *both plus*, and that of the *southerly* point *minus*, if, without any change of intensity, the direction of the terrestrial force deviated towards the *west*. From the second part it appears, that if the direction of the terrestrial

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* At this point, if the needle was much agitated, it moved towards east.

force urging the north end of the needle deviated towards the *east*, without changing its intensity, the deviations of the *westerly* and *easterly* points of equilibrium would be *both minus*, and that of the southerly point *plus*. These agree precisely with the conclusions I had previously drawn.

In order to point out the effects of increasing or diminishing the forces which the magnets exerted on the needle, compared with the terrestrial forces, or the effects that would take place if, without changing their direction at any particular time, the terrestrial forces decreased or increased in intensity, from both the last positions of the axis of the magnets, namely, N 0° 40′ E and N 0° 50′ W, I brought the magnets nearer to the centre, keeping their axes still in the same line, and observed the corresponding changes in the points of equilibrium at every $\frac{1}{10}$ inch by which they approached the centre of the needle.

Table of the Points of Equilibrium corresponding to several distances of the Magnets from the centre of the Needle, the angle between the axis of the Magnets and the meridian remaining the same.* 11 *May*, 9h *A. M. to Noon ; compass No. I.

Angle between the axis of the magnets and the meridian.							
N. 0° 40′ E.				N. 0° 50′ W.			
Distances.	Points of Equilibrium.			Distances.	Points of Equilibrium.		
	° ′	° ′	° ′		° ′	° ′	° ′
16.95	N. 33 36 W.	no point	no point	16.95	no point	N. 31 34 E.	no point
16.85	37 16	no point	no point	16.85	no point	36 20	no point
16.75	41 22	N. 17*20 E.	S. 17 10 W.	16.75	N. 27 40 W.	39 0	S. 16 20 E.
16.65	46 24	30 26	13 16	16.65	34 32	45 38	10 18
16.55	50 08	40 32	10 38	16.55	42 18	49 22	8 40
16.45	54 52	46 16	8 50	16.45	50 04	55 38	7 16
16.35	58 46	52 30	8 12	16.35	54 38	59 0	6 28
16.25	63 18	57 16	7 18	16.25	59 50	64 14	5 16
16.15	70 06	65 34	6 34	16.15	66 14	72 26	5 10
16.05	78 48	74 06	6 12	16.05	74 00	83 00	5 06
15.95	no point.	no point	5 50	15.95	no point	no point.	4 18

* The equilibrium at this point was not stable ; at the opposite point S 17° 10′ W the needle was very steady.

From this it appears, that, as the magnets approached the centre of the needle, or as the intensity of their action *increased*, the *westerly* point of equilibrium proceeded towards the *west*, and the *easterly* towards the *east;* that is, both *receded* from the *north;* or the direction of the deviation of the first was *plus* and of the second *minus.* The *southerly* point of equilibrium, as the intensity *increased*, proceeded towards the *south;* so that when the northern extremity of the axis of the magnets made an angle with the meridian towards the *east*, or in the direction *minus*, the *southerly* point of equilibrium deviated in the direction *plus;* and when the angle was in the direction *plus*, the deviation of the *southerly* point, arising from the *increased* action of the magnets, was in the direction *minus:* that is, as the intensity of the action of the magnets *increased*, the *southerly* point of equilibrium deviated in a direction contrary to that of the angle which the axis of the magnets made with the meridian.

Applying this to the variation of the terrestrial magnetic intensity, supposing that of the magnets constant, as this intensity *decreased*, the *easterly* and *westerly* points of equilibrium would *both recede* from the *north*, and the *southerly* would proceed towards the *south;* as it *increased*, the *easterly* and *westerly* points would *approach* the *north*, and the *southerly* point *recede* from the *south.* Hence the deviation of the *westerly* point of equilibrium being *minus*, and, at the same time, that of the *easterly* point *plus*, would indicate an *increase* in the intensity of the terrestrial force; and the same would be indicated by the *receding* of the *southerly* point from the *south.* If at the same time that the intensity of the terrestrial force *increased*, the direction of that urging the north end of

the needle deviated towards the *west*, the deviation of the *westerly* point of equilibrium from the *increase* of intensity would be *minus*, and from the *change of direction*, *plus* ; so that its character, whether *plus* or *minus*, depending upon the one cause or the other producing the greater effect, would in some cases be somewhat ambiguous. With regard to the *easterly* point of equilibrium, under the same changes of intensity and direction, its deviation from both causes would be *plus ;* and if the intensity and westerly deviation of the force increased during the same period, we should expect little or no ambiguity in the character of the deviation of the easterly point of equilibrium. With regard to the *southerly* point of equilibrium, when that point is towards the *east*, the *increase* of intensity would cause its deviation to be *plus*, and the *change of direction*, *minus ;* so that here again we might expect some ambiguity ; but when the *southerly* point is towards the *west*, these two causes acting at the same time, would both tend to make its deviation *minus*. From this it would appear, that at particular points of equilibrium, to the west of north and to the east of south, if the intensity of the terrestrial force *increased* with the deviation of its direction *westward*, the two causes might so counteract each other's effects that little or no deviation of the needle at these points would be apparent ; but the precise situation of such points would depend in a great measure on the nature of the arrangement of the magnets by which the needle was held in equilibrio at them.

To proceed with the observations on the daily changes in the several points of equilibrium, I have stated that, during the time in which I made observations, in-doors, on the needle No. 1, I had likewise observed, in the open air, the deviations

of another needle, which I call No. 2. This needle is 4.3 inches long, nearly of a similar form to No. 1, and, like it, balanced by an agate cap on a fine point. The rim of the compass-box is accurately divided into degrees, so that I can read to the nearest 5′. The observations with this needle were commenced on the 4th of May, and continued without much interruption until the 18th: these I shall first describe.

Two bar magnets, each ten inches long, .95 inch wide, and .4 inch thick, but not by any means powerful, were placed on the same horizontal table as the compass, with their poles towards those of the needle of the same name. As my object at first was, to observe the changes at points differing considerably in their positions with regard to the north and the south, I did not adjust the magnets in the meridian, but each somewhat inclined to it, the nearest end of each being 11.575 inches from the centre of the needle. I had taken the time of vibration of the needle previously to applying the magnets, but lost the memorandum of it; however, when they were removed, at the conclusion of the observations, it made ten vibrations in nineteen seconds.

My first object was, to determine the three points at which the needle would remain stationary, after being agitated; that is, the three points of *stable equilibrium*, and likewise the three points of *instable equilibrium*: I determined them as under:

4th of May, $5^h 20^m$ A. M.

	The north end of the needle was stationary,	but being agitated, it moved towards
At	S 55° 30′ W	South.
	S 55 40 W	West.
	S 59 0 E	South.
	S 59 45 E	East.
	N 20 50 E	East.
	N 20 10 E	North.

So that the three points of *instable equilibrium* were nearly

S 55° 35′ W, S 59° 20′ E, N 20° 30′ E.

At the three points N 56°40′ W, S 15°55′ W, N 40°55′ E, the north end of the needle, after being agitated, settled steadily. Hence the six points of equilibrium, in their order round the compass, were,

Stable.	Instable.	Stable.	Instable.	Stable.	Instable.
N 56° 40′ W,	S 55° 35′ W,	S 15° 55′ W,	S 59° 20′ E,	N 40° 55′ E,	N 20° 30′ E.

Vibrating the needle from S 60° W, it made six vibrations in thirty seconds about the point N 56° 40′ W; vibrating it from S 54° W it made four vibrations in twenty seconds about the point S 15° 55′ W: I could not obtain the vibrations about the point N 40° 55′ E, on account of its proximity to the instable point N 20° 30′ E.

I have been thus particular in describing the steps which I took previous to the first set of observations, in order that the nature and situation of the three points of *stable equilibrium* may be clearly understood, and to render unnecessary any previous explanation for the other sets of observations.

Tables of the daily changes observed to take place in the points at which a Needle was retained in equilibrio by two bar Magnets.

I.

***The Points of Equilibrium nearly N. 57° W., N. 40° E., S. 16° W.; the axes of the Magnets slightly inclined to the meridian; the distances from the nearest ends of the Magnets to the centre of the Needle,* 11,575 *inches. Compass No.* II.**

May 4.			
Time.	Points of Equilibrium.		
h. m.	° ′	° ′	° ′
6 00	N. 56 40 W.	N. 40 55 E.	S. 15 55 W.
6 30	56 20	40 10	16 15
7 00	56 05	39 30	15 50
7 30	55 40	39 20	15 50
8 00	56 20	39 35	15 45
9 00	56 00	39 20	16 0
10 00	54 10	35 15	16 40
11 00	52 55	27 20	17 40
Noon	53 10	no point	17 40
0 30	52 30	no point	18 10
1 00	53 30	no point	17 55
2 00	55 35	29 15	16 45
3 00	56 45	37 20	16 15
4 00	56 30	36 45	15 55
5 00	57 05	38 30	15 50
6 00	56 40	38 10	15 40
7 00	57 20	40 20	15 20

Here we see that the *westerly* and *easterly* points *approach* and *recede* from the *north together*, or that when the deviation of the *one* is *minus*, that of the *other* is *plus*; that they *approach*, with a slight interruption, betwen seven and eight o'clock, until between twelve and one o'clock, after which they recede, with a slight interruption, about four o'clock, during the rest of the day: and that the *southerly* point *recedes* from the *south* until between twelve and one o'clock, and *approaches* it during the rest of the day, with similar interruptions.

II.

***The Points of Equilibrium nearly N. 36° W., N. 57° E., S. 5° E.; the axes of the Magnets slightly inclined to the meridian; distances 11,575 inches. Compass No.* II.**

Time.	May 5.			May 6.			May 7.		
	Points of Equilibrium.			Points of Equilibrium.			Points of Equilibrium.		
	N. W.	N. E.	S. E.	N. W.	N. E.	S. E.	N. W.	N. E.	S. E.
h m.	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
5 30	37 25	57 00	5 15	34 35	55 45	5 15	—	—	—
6 00	35 00	56 00	5 15	35 00	56 10	5 20	34 40	56 15	4 25
6 30	34 20	55 15	5 15	33 15	55 10	5 20	33 40	53 55	5 30
7 00	34 40	55 45	5 20	32 05	54 40	5 20	30 40	52 55	5 30
7 30	32 30	54 30	5 30	31 25	54 10	5 40	29 20	53 05	5 00
8 00	30 25	53 55	5 30	28 40	52 50	5 45	23 30	51 35	5 25
9 00	no point	49 06	6 36	23 30	50 55	5 35	no point	47 35	5 40
10 00	no point	47 34	5 44	no point	49 40	5 30	no point	45 30	5 40
110 0	no point	48 00	6 16	no point	48 20	5 15	no point	46 00	5 46
Noon	no point	47 30	5 00	no point	47 15	5 05	no point	45 20	6 00
0 30	no point	47 00	5 00	no point	47 30	5 05	—	—	—
1 0	no point	46 35	5 15	no point	48 10	5 05	no point	47 06	5 20
1 30	no point	46 10	5 00	—	—	—	—	—	—
2 00	—	—	—	—	—	—	no point	48 45	5 30
3 0	31 15	52 55	4 15	30 30	52 55	4 30	no point	50 30	5 00
4 0	32 00	54 05	4 40	31 36	52 10	4 40	23 30	51 20	5 30
5 0	31 30	52 34	5 50	30 40	52 30	5 15	29 40	52 40	5 15
6 0	32 00	54 00	4 05	30 00	54 00	4 30	32 15	54 00	5 00
7 0	32 30	54 25	4 45	29 40	53 30	5 00	29 00	53 55	5 35
8 0	34 30	54 55	5 05	32 30	54 34	5 05	32 15	55 40	5 30
9 0	35 40	55 00	5 20	36 30	56 30	5 00	33 25	55 00	5 40
10 0	—	—	—	34 30	55 16	5 30	34 45	55 15	5 20
11 0	36 30	55 20	5 30	35 20	55 55	5 05	—	—	—

Here it appears that the *easterly* and *westerly* points *approach* and *recede* from the *north together*, with only five exceptions, viz. May 6th, at 4h 5h and 6h P. M.; 7th, at 7h 30m A. M. and 9h P. M. The direction of the deviation of the *easterly* point is *minus* till the middle of the day nearly regularly, and is *plus*, during the remainder, with some irregularities in the afternoon. Though the character of the deviation of the *southerly* point is somewhat ambiguous, as we have seen would be the case in this situation, if the changes of direction and of intensity counteracted each other's effects, yet, with the exception of some irregularities, it agrees with that of the observations at the south, from the 20th to the 27th of April, the maxima being however rather later.

III.

The Points of Equilibrium nearly N. 19° W. N. 10° E. S. 3° W.; the line of the axes of the Magnets very nearly in the meridian; distances 11.725 inches. Compass No. II.

Time.	May 10.			May 11.			May 12.			May 13.			May 14.		
	Points of Equilibrium.			Points of Equilibrium.			Points of Equilibrium.			Points of Equilibrium.			Points of Equilibrium.		
	N. W.	N. E.	S. W.	N. W.	N. E.	S. W.	N. W.	N. E.	S. W.	N. W.	N. E.	S. W.	N. W.	N. E.	S. W.
h. m.	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
6 0	—	—	—	15 55	11 15	1 55	15 10	5 50	4 30	16 0	10 0	3 15	17 50	not stable	5 35
7 0	10 15	15 30	S. 4 40 E.	15 15	13 30	S. 0 40 E.	14 10	5 30	2 10	19 30	16 40	1 25	18 0	13 30	7 20
8 0	8 20	13 45	S. 2 45 E.	16 40	16 15	S. 0 25 E.	12 40	no point	2 40	17 20	12 0	1 25	13 45	14 30	S. 0 10 E.
9 0	4 5	2 45	S. 4 45 E.	16 0	13 45	S. 0 30 W.	10 0	no point	2 0	15 50	not stable	3 35	14 50	12 30	S. 0 45 W.
10 0	10 0	no point	no point	18 55	11 40	4 30	6 0	no point	S. 0 30 E.	11 0	no point	no point	11 50	no point	no point
11 0	13 50	no point	no point	19 50	not stable	9 50	15 20	no point	no point	8 40	no point	no point	15 5	no point	no point
Noon.	16 15	no point	no point	21 30	no point	no point	14 0	no point	no point	12 30	no point	no point	15 55	no point	no point
1 0	16 30	no point	no point	16 5	no point	no point	16 0	no point	no point	10 35	no point	no point	13 25	no point	no point
2 0	15 20	no point	no point	18 50	no point	no point	15 40	no point	no point	17 50	no point	no point	18 0	no point	no point
3 0	17 20	no point	no point	17 20	no point	no point	16 45	no point	no point	17 35	no point	no point	19 10	no point	no point
4 0	15 0	not stable	not stable	17 45	no point	9 30 scarcely stable.	17 10	no point	no point	18 0	no point	no point	19 20	no point	no point
5 0	—	—	—	16 30	9 15 Instable	9 15	18 15	no point	no point	18 0	no point	no point	19 25	no point	10 20
6 0	17 30	12 15	S. 2 30 W.	16 20	10 10	4 30	18 50	no point	no point	18 40	no point	no point	19 40	no point	6 20
7 0	18 40	10 15	3 40	17 25	11 30	2 30	18 40	9 10	S. 3 10 W.	19 30	no point	no point	21 5	8 40	5 15
8 0	18 40	9 30	3 25	18 40	12 40	2 0	20 0	13 20	3 0	21 0	11 0	4 40	22 25	15 50	4 55
9 0	—	—	—	16 0	15 20	3 30	19 30	15 0	2 0	21 30	14 30	4 0	22 5	13 50	4 40
10 0	—	—	—	17 40	14 35	8 40	17 0	18 35	S. 0 5 E.	—	—	—	22 55	10 0	6 0
11 0	18 40	11 15	2 25	—	—	—	19 50	16 20	S. 1 30 W.	—	—	—	—	—	—

In this position of the magnets, their forces so very nearly balance the terrestrial forces, that the *southerly* and *easterly* points of equilibrium are soon lost in the *westerly*, the character of which is in consequence, as we might expect, somewhat ambiguous. The *easterly* point, when it could be observed in the morning, *approached* the *north* from between seven and eight o'clock until it was lost in the *westerly* point; and, from its first appearance in the afternoon, it *receded* from the *north* till the evening, except on the tenth, when it again *approached* the *north* previous to its finally receding. With respect to the deviation of the *southerly* point, its direction appeared to be *plus* till about eight o'clock in the morning, after which it was *minus* as long as it could be observed; and when again observed it had become *plus*: which, as far as the observations go, agrees with those at the south, before mentioned.

After making the observations at 8^h P. M. on the 14th of May, pins being fixed by the sides of the magnets so that I could make them recede from the needle in the same line, I drew them back by very small distances, to see whether, from this point, an increase of the terrestrial force would give changes in the points of equilibrium of the same nature as those which had taken place in the preceding observations: I found them to be as under.

14th May, 8^h 15^m P. M.

Distances.	Points of Equilibrium.		
11.725	N. 22° 25′ W.	N. 15° 50′ E.	S. 4° 55′ W.
11.742	21 00	10 30	6 40
11.760	19 35	{ 7 40 not stable }	6 55
11.775	18 30	no point	8 00

The agreement in the general character of these with that of the observations from five o'clock till eight, will clearly lead us to infer that the changes observed in the positions of the points of equilibrium during that time, arose from a *diminution* of the terrestrial forces, as well as a change in their directions: had the agreement in all the observations themselves been complete, we must have inferred that *diminution* of intensity was the sole cause of the changes. After these observations, the magnets were restored to their former distances for those at nine and ten o'clock.

IV.

The Points of Equilibrium nearly N. 25° W., N. 25° E., S. 3° E.; the line of the axes of the Magnets coinciding with the meridian; distances 11.655 *inches. Compass No.* II.

Time.	May 15.			May 16.			May 17.		
	Points of Equilibrium.			Points of Equilibrium.			Points of Equilibrium.		
	N. W.	N. E.	S. E.	N W.	N. E.	S. E.	N. W.	N. E.	S. E.
h. m.	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
6 00	*13 10	19 30	4 40	——	——	——	21 45	24 35	3 00
7 00	*12 35	17 45	6 45	23 00	26 40	4 05	20 05	23 40	4 20
8 00	*no point	15 00	no point	21 50	25 40	5 20	18 35	24 00	5 05
9 00	13 10	17 10	5 05	23 00	26 45	5 40	18 55	22 35	3 55
10 00	11 00	15 00	6 50	23 00	25 35	4 00	19 05	22 15	3 20
11 00	——	——	——	20 30	21 30	3 10	18 00	19 40	3 00
Noon.	10 20	13 00	2 00	19 40	18 50	0 50	22 05	20 20	0 55
1 00	17 50	15 25	0 15	22 25	19 40	S. 1 10 W.	21 30	21 05	0 50
2 00	19 10	15 15	0 50	21 15	18 45	S. 0 15 W.	21 00	20 30	1 26
3 00	20 00	17 50	0 40	20 55	19 35	S. 0 20 E	20 00	21 25	3 20
4 00	20 20	20 40	2 30	21 00	21 00	1 20	19 50	20 00	2 55
5 00	21 00	22 20	3 10	21 20	22 30	2 15	21 10	21 50	2 00
6 00	22 00	24 00	3 00	22 30	24 00	2 25	21 30	22 40	2 20
7 00	23 50	24 40	2 00	——	——	——	22 54	24 30	2 40
8 00	25 15	26 20	2 25	——	——	——	24 00	26 40	3 20
9 00	25 15	26 20	2 00	23 10	26 55	4 35	26 20	27 00	2 00
10 00	25 00	26 05	2 05	24 50	25 06	2 26	26 36	28 05	3 00

* These observations were made with the distances 11.725 inches.

From these we see, as before, that the *westerly* and *easterly* points *approach* and *recede* from the *north together*, with a few exceptions, which happen about eight o'clock in the morning and three o'clock in the afternoon. The nearest *approach* of these points towards the *north*, or their *greatest* deviations in the directions *minus* and *plus*, appeared to take place about noon. The changes in the *southerly* point again agree very nearly with the observations at the south from the 20th to the 27th of April: the *maximum* in the direction *minus* appearing to happen about an hour after noon.

The following observations were made with the compass No. *I*, having the magnets adjusted on the same horizontal table, in the open air, as I have described previous to the observations with it on the 11th of May. During the same time that these were made, I likewise made those with the compass No. *II.*: the two sets were made in the following order; those with No. *II.* were begun a few minutes before the hour, so as to finish those with No. *I.* at nearly the same time after it, the whole time occupied being, in general, about ten minutes.

V.

The Points of Equilibrium nearly N. 80° *W.; N.* 80° *E.; S.; the line of the axes of the Magnets coinciding with the meridian; distances* 15.95 *inches. Compass No.* 1.

Time.	May 14.			May 15.			May 16.			May 17.		
	Points of Equilibrium.			Points of Equilibrium.			Points of Equilibrium.			Points of Equilibrium.		
	N. W.	N. E.	S. E.	N. W.	N. E.	S. E.	N. W.	N. E.	S. E.	N. W.	N. E.	S. E.
h. m.	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
6 00	83 20	83 14	1 06	not stable	not stable	0 52	——	——	——	75 36	75 28	0 56
7 00	80 08	79 36	1 22	83 18	83 26	0 42	77 50	77 34	0 46	74 28	74 22	0 54
8 00	80 14	79 26	1 16	78 46	78 36	0 44	76 00	76 16	0 52	73 48	73 24	0 58
9 00	83 12	84 58	1 06	77 54	77 24	0 34	77 42	77 52	1 22	73 52	73 28	0 46
10 00	81 46	80 18	0 52	77 20	77 52	0 40	77 12	77 20	1 04	73 32	73 24	0 46
11 00	79 02	77 24	0 36	——	——	——	73 36	73 18	0 54	73 06	72 52	0 40
Noon	78 44	77 28	0 14	70 40	69 18	0 18	73 06	72 48	0 30	74 48	73 44	0 18
1 00	78 52	78 32	0 18	71 40	70 34	0 18	73 32	72 50	0 22	74 10	73 08	0 24
2 00	78 38	77 20	0 34	69 48	68 50	0 18	72 18	71 52	0 24	72 06	71 36	0 14
3 00	77 32	78 00	0 36	70 54	70 00	0 32	72 52	72 28	0 24	71 40	71 08	0 40
4 00	79 00	78 24	0 42	72 10	71 44	0 42	73 04	72 54	0 34	71 46	71 02	0 34
5 00	81 18	81 04	0 56	73 40	72 52	0 50	74 16	74 04	0 34	73 06	72 38	0 34
6 00	83 12	82 22	0 52	74 52	74 20	0 42	75 10	75 04	0 30	73 08	73 06	0 40
7 00	87 08	88 34	0 52	76 18	75 08	0 40	——	——	——	75 20	74 46	0 38
8 00	89 40	89 40	0 36	78 38	77 08	0 32	——	——	——	77 12	76 30	0 54
9 00	not stable	not stable	0 34	78 00	77 44	0 20	76 56	77 26	0 56	79 00	78 50	0 44
10 00	no point	no point	0 34	77 50	77 32	0 22	77 28	77 10	0 50	79 44	79 30	0 42

In these the *westerly* and *easterly* points *approach* and *recede* from the *north together*, almost without exception. The *maximum* deviation of the *westerly* point in the direction *minus*, and of the *easterly* point in the direction *plus*, here take place between two and three o'clock in the afternoon. The changes in the *southerly* point again agree very nearly with the observations in April; the *maximum* in the direction *minus* still happening about an hour after noon.

I have shown, by the observations made on the 11th of May, that a change in the *directions* alone of the terrestrial

forces, would cause the deviations of the *easterly* and *westerly* points of equilibrium to be always *both plus* or *both minus*, that is, that the *one* would always *approach* the north whilst the *other receded* from it; that the effect of a change of *intensity* alone of these forces was, to make the deviation of the *one point plus*, whilst that of the *other was minus*, or to make them *approach* and *recede* from the north *together;* and that if a change took place at the same time both in the *intensity* and direction, the *westerly* and *easterly* points might *both approach* and *both recede* from the north, or the *one approach* and *the other recede*, according as the one cause or the other produced the greater effect; but that their *both approaching* or *both receding* from the *north*, at any time, must arise from a change of *intensity*. Comparing then the whole of the preceding observations with these effects, it is evident that the changes which take place cannot be explained by a change in the directions alone of the terrestrial forces, but that their characters agree, as nearly as we can possibly expect, with the effects that would take place from an *increase of intensity* at the time that the direction deviated towards the *west:* we are therefore led to infer, that such an *increase of intensity* must take place in the terrestrial force during the time of the *westerly deviation*. The change of intensity during the day, has been already ascertained by the observations of HANSTEEN on the vibrations of a needle very delicately suspended, but in the present state of our information respecting the magnetical phænomena of the earth, the series of observations which I now present will not, I trust, be considered without interest, although they should, at first sight, appear only to confirm results obtained from previous observations.

At different times on the 14th of May, after observing the directions of the needle No. *I*, I found its time of vibration at the south point of equilibrium. This was not done with the expectation of being able to draw any very decided conclusions from these observations, since the suspension of the needle was not sufficiently delicate to admit of its making many vibrations under a diminished force, nor had I then more accurate means of ascertaining the time than a good watch with a seconds' hand; but to see how far the change in the ratio of the intensities of the terrestrial forces and those of the magnet would be sensible. The times required for eight vibrations of the needle at different hours in the day were as follow:

	Vibrations.	″
At 6^h A. M.	8	in 50
10	8	52
Noon	8	52
1 P. M.	8	52
4	8	50.4
9	8	50

The differences here are such, that, even with the means I possessed, they could not arise from errors in the observations, and they point distinctly to a diminution, during the middle of the day, of the forces tending towards the point about which the vibrations were made; that is, to an increase in the ratio of the terrestrial forces to those of the magnet. With a very delicate suspension, such as Hansteen made use of, and very accurate means of measuring the time, I consider that the times of vibration of a needle so adjusted would give a ready and accurate measure of the ratio of the terrestrial forces to those of the magnet, and, if taken under precisely

the same circumstances with respect to the magnets, of the intensity of the terrestrial forces.

Previous to giving the observations, I have considered what would be the case if, at the same time that the direction of the force deviated towards the west, its intensity increased; but from the observations of Hansteen this does not appear to be precisely the case. He found the minimum intensity to take place about ten o'clock in the morning, that is, about two hours and a half after the westerly deviation had commenced; and the maximum intensity about the same time after the return towards the east. This increase of the intensity during the latter part of the westerly deviation will account for the general character of the changes; and the difference in the times at which the changes of intensity and direction take place, may possibly be pointed out by some of the apparent anomalies in the various changes observed at different points.

Having arrived at these conclusions from my observations, I intended to close them for the present, with those I have last given, but as I had not removed the compasses, I again observed the directions on the following day, and I soon found that changes took place to an extent I had never before noticed. In consequence of this I continued to observe during the day, and felt much gratified that I still had the opportunity, as by this means I discovered that I had not noticed a circumstance, which must in all cases affect the extent of the deviations, and must be allowed for when the effects arising solely from the changes in the terrestrial forces are to be considered. In my first observations I had not noticed the temperature of the room in which they were made; and although I had noticed that the state of the weather appeared to have an

influence on the extent of the deviations, I did not consider that such changes of temperature as they were liable to would sensibly affect the energy of the magnets themselves.

In the experiments of CANTON, on the effects of temperature in increasing or diminishing the forces of magnets, the precise change of temperature in the magnets could not very well be ascertained from the manner in which it was produced, but it could not be less than 80° or 90° on FAHRENHEIT'S scale, and yet this only caused the needle to move from N 45° E to N 44¼° E, when applied to a single magnet retaining the needle in this position; and when two magnets were applied on contrary sides of the needle, so that it pointed due north between them, an increase of temperature to this extent, in either of the magnets, only caused it to deviate 2¾° towards the other. From this we might be led to suppose that in such observations as I had been engaged in making, the effects of small changes of temperature might be neglected without sensible error, and that even in extreme cases the deviations would not be materially affected: the latter, however, I found was far from being the case.

I had continued to observe in the open air under the same impression which I had when the compasses were in a room, but, on the day in question, the effects of a hot sun upon the magnets were too unequivocal to be doubted. So early as seven o'clock in the morning I had found a deviation of 13° 20′ of the western point, and 11° 48′ of the eastern, towards the north, from their situations at $5^h\,15^m$ in the compass No. *I.*; and in No. *II*, 6° 55′ of the westerly and 4° 20′ of the easterly point: whereas on the preceding day, the corresponding changes from the night before had been, for No. *I*, 2° 54′ and

2° 58′; and for No. II, 5° 35′ and 1° 55′. The effects afterwards corresponded with these, but they will be best understood from the observations themselves.

Compass No. I. *May* 18.

Time.	Points of Equilibrium.			Remarks.
	N. W.	N. E.	S. E.	
h. m.	° ′	° ′	° ′	
5 15	80 40	79 38	0 52	
6 00	72 36	72 46	1 06	
7 00	67 20	67 50	1 16	Clear sun, striking hot upon the magnets.
8 00	62 38	63 04	1 08	
9 00	64 26	64 48	1 00	The magnets somewhat sheltered by trees.
10 00	65 36	65 42	0 52	
11 00	52 28	50 56	1 04	The magnets had become hot.
Noon.	45 22	44 16	0 42	
1 00	49 32	49 14	0 06 W.	Sun obscured.
2 00	50 26	50 24	0 18 E.	
3 00	51 26	51 34	0 32	
4 00	51 52	51 48	0 30	Sun faintly out.
5 00	53 20	53 24	0 30	
6 00	54 00	54 06	0 26	
7 00	54 20	54 30	0 32	Cold evening.
8 00	55 40	55 50	0 36	
9 00	56 20	56 28	0 38	

Comparing the changes here with those of the day before, we have:

	Changes of direction.					
	May 18.			May 17.		
	W. point.	E. point.	S. point.	W. point.	E. point.	S. point.
	° ′	° ′	° ′	° ′	° ′	° ′
From the morning till the middle of the day -	35 18	35 22	1 22	3 56	4 26	0 44
From the middle of the day till night - -	10 58	12 12	0 44	8 04	8 28	0 40

The effect produced on the magnets by the heat of the sun appears to have been of a permanent character, since, although

the evening was cold, the needle did not return to within 24° and 23° of its situations in the morning. When the sun was hottest, a thermometer exposed to its rays stood at 97° FAHRENHEIT, so that the change in the temperature of the magnets could not exceed 50°.

On the day following, a thermometer being placed on the table with the bulb exposed close to the side of one of the magnets, I observed as follows:

Time.	Ther.	Points of Equilibrium.	
		N. W.	N. E.
h.	°	° ′	° ′
3 P.M.	73	50 28	50 58
9 P.M.	51.5	54 52	55 08

Here, although the magnets experienced a change of temperature of 21°,5, yet the changes of direction were only 4° 30′ and 4° 10′.

The observations with the other compass were of the same character on the 18th of May as those with No. I.: they were as follow:

Compass No. II, *May* 18.

Time.	Points of Equilibrium.			Remarks.
	N. W.	N. E.	S. E.	
h. m.	° ′	° ′	° ′	
5 10	26 25	28 40	3 15	Clear sky.
6 00	19 40	23 15	4 55	
7 00	19 30	24 20	5 05	
8 00	17 45	22 55	6 00	Clear sun, striking hot upon the magnets. Therm. in shade 63°; exposed to the sun 97°.
9 00	no point	13 00	no point	
10 00	no point	5 15	no point	

Finding the deviations so much increased and the magnets hot, I covered them over with porous earthen pans, pouring

cold water on the pans to reduce the temperature; and had at

Time.	Points of Equilibrium.			Remarks.
		N. E.		
h. m.		° ′		
11 0	no point.	7 20	no point.	I now uncovered the magnets.
11 15	no point.	1 45	no point.	
11 30	no point.	0 40	no point.	

The needle now appeared to have scarcely any directive power, since, when agitated again, it moved to N 1° 15′ W, and at 11ʰ 45ᵐ, on being agitated, it settled at N 2° 15′ W.

At this time I covered the magnets with blotting paper, to retain the water, and poured on them, for some minutes, water at the temperature 55°, after which I had at

	N. W.	N. E.	S. W.	
h. m.	° ′	° ′	° ′	
11 52	18 00	14 40	0 20	I removed the paper from the magnets.
0 55	14 55	9 20	0 20	
1 12	10 20			

When the needle was at this point I again covered the magnets with paper, and poured water nearly boiling on them; when the needle went rapidly to 0°, then returned and stood steadily at N 1° 20′ W. I again reduced the temperature by pouring water at 55° on the magnets, and had, at

h. m.	N. W.	W. E.	S. W.
1 25	17 10	14 10	0 10
The needle pointing		14 10	

I again poured hot water on the magnets; when it went rapidly to 0°, N 3° W, and, when continually agitated, stood at N 0° 40′ W, but a few minutes

afterwards at N 1° 20′ W. On cold water being poured on the magnets, they now only recovered their power so far that their forces were very nearly equal to the terrestrial forces, since the needle appeared to have little or no directive power either at north or at south: it would bear almost any degree of agitation at several positions within 3° or 4° on either side of these points. On the following morning at seven o'clock the magnets had recovered rather more power, the needle then pointing N 9° 10′ E.

In these observations the effects of increase of temperature, in diminishing the power of the magnets, are most decided, and certainly much beyond what could have been anticipated from the experiments of CANTON. The greatest change of temperature which these magnets underwent, could not exceed that to which his were subjected, and the power exerted on the needle by his, must have been fully equal to that of the magnets here made use of. The angle at which the forces were exerted, in the one case or the other, would have an effect, but not, as appears to me, equal to the difference in the deviations in the two cases. On this, however, I cannot speak decidedly, as I can at present only refer to the abridged account of his experiments. Another difference in the effects which took place in the two cases is, that after some hours, the magnets, which CANTON made use of, recovered their power; whereas, in the present instance, a considerable portion appeared to be permanently destroyed, although in the case of those applied to the compass No. I, they were not subjected to a heat by any means so great as those were which he employed, since the temperature of these could never have exceeded 100° FAHRENHEIT. After sixteen days, they now

produce only the same effect at the distance 15.25 inches which they previously produced at the distance 15.95; but during the whole of this time they appear to have exerted the same, or very nearly the same energy at the same temperature. It would appear then, that the permanent destruction of their power must have arisen from their being heated beyond a certain degree, and it does not seem improbable, that solar heat may have a greater influence than any other in producing such an effect.

Seeing then that such is the effect of the temperature of the magnets on the changes which take place in the points of equilibrium, it might perhaps be supposed that variations in it, if not the only, were the principal causes of the peculiarities which I have pointed out. Although I felt persuaded that this was not the case, I delayed presenting these observations until I had seen such peculiar effects taking place independent of the temperature of the magnets. For this purpose, I have been observing the changes in the points of equilibrium, in a situation where the magnets were exposed to very small variations in temperature, the greatest during eleven days not having exceeded 8° Fahrenheit, and not 4° during the observations of any day. The changes in the temperature of the magnets I regularly noted, and expected to see, in perhaps some few instances, that the changes in the situations of the points of equilibrium would be in opposition to those which would arise from the change of temperature in the magnets alone. My expectations have been more than answered; since I have noticed this to take place, not in a few instances, which, being decided ones, would have been sufficient to have established the principle, but repeatedly in the observations of the same day.

It will be remembered that, the effect of an *increase of temperature* in the magnets, by *diminishing the intensity* of their forces, is to make *both* the *westerly* and *easterly* points of equilibrium *approach* the *north*, and that a *diminution of temperature* has the effect of making them *both recede* from it, independent of any other cause. Now, in these last observations, it has only happened on one day that I have not seen repeatedly *both* points *approach* the *north* when the *temperature* of the magnets has *decreased*, and *both recede* from the north with an *increase of temperature;* showing clearly that the tendencies in one case of *both* points to *approach* the *north*, and of *both* to *recede* from it in the other, were sufficient to counteract the contrary tendencies arising from the changes of temperature in the magnets; and proving that the peculiar changes which I have noticed, although modified by the temperature of the magnets, were not the effects of it.

I do not propose at present to give these observations, as it is my intention to make a series on the precise effects of changes of temperature in the magnets, so as to be able to free the observations entirely from such effects, by reducing them to the same standard. When this is done, I shall no doubt find, that the extent of the deviations which I have obtained require some correction, chiefly in the observations with the needle out of the meridian; and I likewise expect that here the times of the maxima deviations will be found nearer to the times of the maximum intensity and minimum intensity, as determined by HANSTEEN; but the leading fact, of the *westerly* and *easterly* points *approaching the north at the same time*, and *receding from it at the same time*, during certain periods, will remain unaltered. Thus the conclusions which I have before drawn respecting the increase and decrease of intensity in the

terrestrial forces will not be materially affected by the changes which may have taken place in the temperature of the magnets during the times of observation. With regard to the observations in the meridian, that is, with the needle pointing *north* or *south*, they could not be much affected by changes in the temperature of the magnets, especially by such small changes as I am persuaded alone took place: the extent of the deviations may, in some instances, have been slightly increased or diminished, but their directions could not be changed.

I have before mentioned that both Mr. Barlow and myself found some anomalies between the observations in-doors and those at the same points in the open air. When the times of the maxima shall have been determined independent of the temperature of the magnets, I expect it will be found that these anomalies have arisen from the difference in the changes of temperature in the magnets when in-doors and when in the open air: of this I only feel that degree of doubt, which should always be entertained until a fact is established.

If such observations as I have given were continued for a length of time, particularly those near the east and west, I certainly expect that they would lead to important conclusions respecting the causes of the diurnal variation, and I regret that, as I have not the time to devote to them myself, I must leave them to be made by others possessed of more leisure. Should they be undertaken, the necessity of ascertaining in the first instance, the effects which changes of temperature have on the forces of the magnets employed, and of observing the temperature of the magnets themselves

when the directions of the needle are taken, is here clearly pointed out.

The striking effects which I have seen to arise from a change of temperature in the magnets have certainly led me to adopt the opinion, that temperature, if not the only cause of the daily variation, is the principal. This was the opinion of CANTON, but he could not, by it, account for the morning easterly variation. I might here offer some conjectures on this subject, but as it is not my intention at present to enter fully into the general question of the cause of the daily variation, I will defer them, at least until I shall have ascertained the precise effects of changes in the temperature of magnets.

XXV. *On Fossil Shells. By* Lewis Weston Dillwyn, *Esq. F. R. S. In a Letter addressed to the Right Honourable Sir* Humphry Davy, *Bart. Pres. R. S.*

Read June 5, 1823.

My Dear Sir,

As fossil shells are more numerous, and generally occur in a better state of preservation than any other of the organic remains, they have become one of the most interesting objects for geological research, and there is such an exact conformity in the structure of many of these fossils with the living genera, as to render it in the highest degree probable, that the habits of their animals were also similar. By availing ourselves of these analogies, some circumstances attending the distribution of fossil shells may be observed which have hitherto escaped notice, and if you should find them to be sufficiently interesting, or likely to open a new door for enquiry, I beg that you will submit to the Royal Society the following observations on the fossil remains of the Molluscæ.

Pliny, in describing the shell fish which was supposed to yield the Tyrian die, has observed, ‘lingua purpuræ longitudine digitali, qua pascitur perforando reliqua conchylia;’ and Lamarck says, that all those molluscæ whose shells have a notch or canal at the base of their apertures, are furnished with similar powers, by means of a retractile proboscis; and in his arrangement of invertebral animals they form a section of the Trachelipodes, with the name of ‘Zoophages.’ Whether all these Trachelipodes are possessed of the same predaceous powers of boring into hard substances, and whe-

ther some of them may not subsist chiefly on dead animals, my own observations have led me greatly to doubt; but this notch or canal is made for the protrusion of a trunk, which is formed to answer the same purposes as the respiratory organs of a Gastrobranchus,* and may serve at once to distinguish a carnivorous species. The following fossil genera belong to this section of the Trachelipodes—Conus, Oliva, Ancilla, Terebellum, Seraphs, Cypræa, Ovula, Volvaria, Marginella, Voluta, Mitra, Terebra, Buccinum, Harpa, Monocerus, Purpura, Cassis, Cassidaria, Strombus, Rostellaria, Triton, Murex, Ranella, Pyrula, Fusus, Cancellaria, Potamides, and Cerithium.

In all the other genera of turbinated univalves, the lower margin of the aperture, instead of being either notched or channelled, is entire; and ADANSON, in his History of Senegal, so far back as 1757, has shown that the Molluscæ of these shells have jaws which are formed for feeding on vegetable substances; and they have been proved, by subsequent observations, to be entirely herbivorous, i. e. the marine genera feed on algæ, and the fresh water and land genera on the leaves of vegetables. These together constitute the other section of the Trachelipodes, which LAMARCK has called ‘Phytiphages,’ and it comprises the following genera of fossils—Turritella, Turbo, Cirrus, Euomphalus, Trochus, Solarium, Delphinula, Scalaria, Natica, Nerita, Ampullaria, †Vivipara, Paludina, Melania, Planorbis, Cyclostoma, Auricula, Tornatella, Bulimus, Helicina, and Helix.

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* See Sir E. HOME's observations on this animal under the name of Myxine, in the Philosophical Transactions for 1815, p. 261.

† I am unable to distinguish this genus from Paludina; and the name of Vivipara is calculated to mislead, for none of the species are more than ovi-viviparous.

Every turbinated univalve of the older beds from transition lime to the lias, which I have been able to procure, or of which I can find any record, belongs to these herbivorous genera, and the family has been handed down through all the successive strata, and still inhabits our land and waters. On the other hand, all the carnivorous genera abound in the strata above the chalk, but are comparatively extremely rare in the secondary strata, and not a single shell has been detected in any older bed than the lower oolite. As a proof of this rarity it may be remarked, in the list of British fossils which Mr. PARKINSON has given in his Introduction to the Study of Organic Remains, that not one single species of either of the carnivorous genera has been referred to any stratum below the London clay, and only the few following species appear in any of the numerous lists of the secondary strata which are given in CONYBEARE and PHILLIPS' Outlines of Geology, viz. a Murex* and *Pleurotoma rostrata* in the green sand, *Cerithium melanoides* in chalk marle, and a few species of Rostellaria in various strata from chalk marle to the lower oolite. For the Pleurotoma and the Cerithium, a reference to the Mineral Conchology is given; and Mr. SOWERBY there only says that he has seen an imperfect cast, very like the former, from the canal at Devizes; and of the latter, that it was found in the London clay, and in the clay *above* the chalk at Newhaven. It is also worthy of remark, that all the above-mentioned Rostellariæ which have been found in secondary strata are nearly allied to the Linnæan

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* Mr. GEORGE SOWERBY has sent me this shell with the name of *Murex calcar*, and if I am not much mistaken, I have seen another species of Murex from the green sand in the extensive collection of Mr. J. S. MILLER.

Strombus Pes Pelecani; and it may be observed that this species, when fully grown, has not any open canal at its base; and that in the figure which Muller has given of the animal there is no appearance, nor in Montagu's description is any mention made, of that retractile proboscis or respiratory trunk, which are the distinguishing characters of a carnivorous Trachelipode. I therefore propose to remove these Rostellariæ of the secondary strata, which are readily distinguished by the remarkable expansion of their outer lips, to form a separate genus with Petiver's name of Aporrhais and the other fossil Rostellariæ which have the recent *Strombus fissus,* for their type are only to be found in strata above the chalk.

Small circular holes, which have been bored by the predaceous Trachelipodes, are frequently found in recent shells, and I have seen exactly similar holes in many fossils, but they have all been taken from the London clay or crag; nor have I been able to find any such appearance in any fossil of the older formations. If this observation should be confirmed by a more extended examination of other cabinets, it will prove that neither the Aporrhaides, or any of those few undoubtedly carnivorous species which have been found in the secondary formations, were furnished with any such predaceous powers as Pliny has described, and that they belong to a subdivision of the Trachelipoda zoophaga, which feed only on dead animals. Without attempting to distinguish the more predaceous from these other genera, I shall however at present content myself with proving, and for this I have adduced sufficient evidence, that the whole family of the carnivorous Trachelipodes are extremely rare in all those strata where the Ammonites and other Nautilidæ abound.

In describing the Ammonites, DE MONTFORT, in his Conchologie Systematique, observes, that they are found of all sizes, "depuis la grandeur d'une Lentille jusqu'a celle de 8 pieds de diametre;" and, as a proof of their great abundance, LAMARCK says, "La route d'Auxerre à Avalon, en Bourgogne, est ferrèe avec des Cornes d'Ammon." These Ammonites, as well as most of the other principal multilocular genera, appear to have become extinct in our northern latitudes when the chalk formation was completed; but a few of the Nautilidæ still inhabit the southern ocean, and their molluscæ belong to the carnivorous order which LAMARCK has described under the name of Cephalopodes. From the occurrence in such great numbers of the carnivorous Trachelipodes in the formation above the chalk, it therefore appears, that the vast and sudden decrease of one predaceous tribe has been provided for by the new creation of many genera, and a myriad of species possessed of similar appetencies, and yet formed for obtaining their prey by habits entirely different from those of the Cephalopodes.

It may be farther observed, that all the marine genera of the herbivorous Trachelipodes to which either of the fossil species belongs, are furnished with an operculum, and that the few carnivorous species which have been found in the secondary strata, agree with them in this particular, although the unoperculated genera are very abundant in the London clay. LAMARCK, of the fresh water Trachelipodes says, that those which are not furnished with an operculum are formed for the occasional respiration of air; but I believe that this observation is not applicable to the marine genera; and it was ADANSON's opinion, that the operculum is intended for the

protection of the animal; nor can I imagine any thing against which such a shield would be more necessary than the long and pliable fingers of the Cephalopodes, when they abounded in the seas, as they must formerly have done. It is, therefore, at least a curious coincidence, that all the marine Trachelipodes of the transition and secondary strata, of which I can find any record, belong to genera which are furnished with an operculum, and that none of the numerous unoperculated genera should have been found in any other than the tertiary formations where the Ammonites disappear. For the protection of the testaceous Gasteropodes no such shield would be wanting, and including this order it may be generally observed, that none of the marine unoperculated Molluscæ, except the Cephalopodes, are to be found in the lias, or in any of its older strata; and it appears to me that a much greater approach towards the same variety of testaceous animals which now inhabits our seas is to be found in the adjoining bed of lower oolite.

The foregoing observations are confined chiefly to British fossils; for as a few of the testaceous Cephalopodes still live in the warmer climates, it is possible that the Ammonites, as well as some others of the extinct genera may have existed longer, and that their remains may be found in the tertiary formations of the more southern latitudes. Although fossil Nautilidæ are common in the secondary strata of the United States, they are said not to have been found in South America; and it may therefore be queried whether the Cephalopodes were not confined to the more northern latitudes when the chalk formation was completed, and whether a decrease in the earth's temperature at that period may not have occa-

sioned the entire destruction of some genera, and a migration of others to the southward.

It is highly probable, when a more perfect knowledge of the testaceous animals has been obtained, that the line of enquiry which I have now suggested may be greatly extended, and the collected tendency of such analogies between the habits of living animals and the organic remains of the different strata, may serve to throw some light on the nature of the changes which the surface of our planet has undergone.

I am, my dear Sir,

Yours very sincerely,

L. W. DILLWYN.

Penllergare,
May 19, 1823.

XXVI. *On the apparent magnetism of Metallic Titanium.* By WILLIAM HYDE WOLLASTON, *M. D. V. P. R. S.*

Read June 19, 1823.

In an account that I lately gave of the properties of metallic Titanium, which is printed in the First Part of the Volume of the Philosophical Transactions for the present year, there is an oversight, which I am desirous of rectifying as soon as may be. I have there stated that the cubic crystals of Titanium, when first detached from the iron-slag where they are found, were all attracted by a magnet, but that when they had been freed from all particles of iron adherent to them, they appeared to be no longer acted upon by it.

Having since that time been led by the observations of M. PESCHIER of Geneva, to examine this question more accurately, I find that, although the crystals are not sufficiently attractile to be wholly supported by the magnet, yet when a crystal is supported by a fine thread, the force of attraction is sufficient to draw it about 20 degrees from the perpendicular, and consequently, that the force of attraction is equal to about one-third the weight of the metal.

When a piece of soft iron of about the same size was made of a cubic form (weighing half a grain), the attractive force of the iron to the same magnet was found, in successive trials, to lift from eighty to ninety times its weight of a silver chain adapted to this inquiry.

By a similar mode of trial, I found that cobalt carried from fifty to sixty times its weight, and that a similar quantity of nickel supported from twenty to thirty times its own weight by the same magnet.

From the above comparison of the magnetic forces, it is evident that the presence of about $\frac{1}{250}$ part of iron as an alloy in the metallic Titanium, would be sufficient to account for this power, without regarding Titanium itself as a magnetic metal; and its origin in the midst of iron, gives every reason to suspect that it would be contaminated by some proportion of that metal.

It is, however, extremely difficult really to detect the presence of so small a proportion of iron, on account of the high colour of the precipitates of Titanium. For though it may be easy to produce an appearance of blue by using a prussiate, which already contains iron, and is consequently better adapted to prove the absence of iron where no blueness appears, than to ascertain its presence, it is by no means easy to obtain the more indisputable evidence of iron by infusion of galls. It is only by repeated evaporation of the muriatic solution, and continued exposure of the residuum to the temperature of boiling water, that I have succeeded in separating enough of the Titanium to allow the blackness of gallate of iron to appear, when the efflorescent edges of the dried salt are touched with infusion of galls.

Although the quantity thus rendered sensible does not appear in proportion sufficient to account for the magnetic force observed, there seems more reason to ascribe it to this impurity, than to suppose Titanium possessed of that peculiar property in a degree so far inferior to the other known magnetic metals.

XXVII. *An account of the effect of Mercurial Vapours on the Crew of His Majesty's Ship Triumph, in the year* 1810. *By* William Burnett, *M. D. one of the Medical Commissioners of the Navy, formerly Physician and Inspector of Hospitals to the Mediterranean Fleet. Communicated by* Matthew Baillie, *M.D. F.R.S.*

Read June 19, 1823.

It has long been known, that in the vacuum of the barometer, mercury rises in a vaporous state at the usual temperature of this climate, and that persons employed in the mines from whence this metal is procured, as well as those who are employed in gilding and plating, have suffered paralytic and other constitutional affections, from inhaling the air saturated with mercurial vapours: had any doubt remained of mercury existing in the state alluded to, it would be effectually removed by the experiments made by Mr. Faraday, detailed in the twentieth number of the Journal of Science, &c.

An unprecedented event, which occurred in one of His Majesty's ships of the line, at Cadiz, in the year 1810, a short time before I took upon me the charge of the Medical Department of the Mediterranean Fleet, has afforded me an opportunity of illustrating this subject on a very extensive scale, the details of which may not, perhaps, be uninteresting to the Royal Society.

The Triumph, of seventy-four guns, arrived in the harbour

of Cadiz in the month of February, 1810, and in the following March a Spanish vessel, laden with quicksilver for the mines in South America, having been driven on shore in a gale of wind and wrecked under the batteries, then in possession of the French, the boats of this ship were sent to her assistance, by which means, during many successive nights, about one hundred and thirty tons of the quicksilver were saved and carried on board the Triumph, where the boxes containing it were principally stowed in the bread-room.

The mercury, it appears, was first confined in bladders, the bladders in small barrels, and the barrels in boxes. The heat of the weather was at this time considerable, and the bladders, having been wetted in the removal from the wreck, soon rotted, and the mercury, to the amount of several tons, was speedily diffused through the ship, mixing with the bread, and more or less with the other provisions. The effect of this accident was soon seen, by a great number of the ship's crew, as well as several of the officers, being severely affected with ptyalism, the Surgeon and Purser being amongst the first and most severely affected, by the mercury's flowing constantly into their cabins from the bread-room; their cabins being, as is usual, on the orlop deck, separated from this store by partitions of wood. In the space of three weeks from the mercury's being received on board, two hundred men were afflicted with ptyalism, ulcerations of the mouth, partial paralysis in many instances, and bowel complaints. These men were removed into transports, where those more slightly affected soon got well; but fresh cases occurring daily, Rear Admiral Pickmore, then in command of the squadron, ordered an inspection to be made by the Surgeons

thereof, and in consequence of their report, sent the Triumph to Gibraltar to remove the provisions, and purify the ship by ablution, the affected men being sent to the Naval Hospital; which order was strictly attended to; the provisions, stores, and likewise the shingle ballast, being removed on shore.

Notwithstanding the removal of the provisions, &c. and afterwards frequent ablution, on re-stowing the hold, every man so employed, as well as those in the steward's room, were attacked with ptyalism; and during the ship's passage, and on her return to Cadiz, the fresh attacks were daily and numerous till the 13th of June, when the Triumph sailed for England.

After their departure from Cadiz they experienced fresh breezes from the N. E.; and the men being kept constantly on deck, the ship aired night and day by windsails, the lower-deck ports allowed to remain open at all times, when it could be done with safety, allowing no one to sleep on the orlop deck, and none affected with ptyalism on the lower deck, a very sensible decrease in the number daily attacked soon became apparent; but nevertheless, many of those already affected became worse, and they were under the necessity of removing twenty seamen and the same number of marines, with two serjeants and two corporals, to a sloop of war and the transports in company. On their arrival in Cawsand Bay, near Plymouth, on the 5th of July, not one remained on the list for ptyalism.

The effects of the mercurial atmosphere was not confined to the officers and ship's company; almost all the stock, consisting of sheep, pigs, goats, and poultry, died from it; mice, cats, a dog, and even a canary bird, shared the same

fate, though the food of the latter was kept in a bottle closely corked up.

The Surgeon (Mr. PLOWMAN) informed me, in conversation, that he had seen mice come into the ward-room, leap up to some height, and fall dead on the deck.

The Triumph, previous to this event, had suffered considerably, by having a number of her men attacked with malignant ulcer, which at one time prevailed to a considerable extent in our ships, both at home and abroad; and in many of the men who had so suffered, the ulcers, which had long been completely healed, without even an erasure of the skin, broke out again, and soon put on a gangrenous appearance.

The vapour was very deleterious to those having any tendency to pulmonic affections: three men died of pthisis pulmonalis, who had never complained, or been in the list before they were saturated with the mercury; and one man who had suffered from pneumonia, but was perfectly cured, and another who had not had any pulmonic complaint before, were left behind at Gibraltar, labouring under confirmed pthisis. Two only out of so large a number affected died from ptyalism, gangrene having taken place in their cheeks and tongue: they had previously lost all their teeth. In the case of a woman, who was confined to bed in the cockpit with a fractured limb, not only were all the teeth lost, but many exfoliations also took place from the upper and lower jaws.

The mercury showed its effects upon the ship herself, by the decks being covered with a black powder; but quicksilver was not discovered at any time in this powder in a native or globular state, though the brass cocks of the boilers,

and the copper bolts of the ship, were covered with the metal, the last to some extent within the wood; a gold watch, gold and silver money kept in a drawer, and likewise some of the iron-work of the ship which had been kept bright, evidently showed the influence of the prevailing atmosphere, being in some places covered with quicksilver.

In a communication with which Mr. PLOWMAN, Surgeon of the Triumph, has obliged me, he states, that those who messed and slept on the orlop and lower decks, with the exception of the midshipmen, suffered equally, while those on the main or upper deck were not so severely affected: the men who lived and slept under the forecastle escaped with a slight affection of the gums. The only reasons which can be assigned for the partial escape of the midshipmen, are, that the windsails were kept always in action, and that these Gentlemen were almost constantly on deck, or were more frequently employed on service out of the ship, in proportion to their numbers, than the men.

Various opinions were entertained of the manner in which the systems of the sufferers were brought under the influence of the mercury. By some, it was supposed to have originated from the use of the bread and other provisions, with which the mercury had mixed itself; and to such an extent was this opinion carried, that I find, by reference to official documents in the Victualling office, seven thousand nine hundred and forty pounds of biscuit were condemned as unserviceable from having *quicksilver mixed with it.*

By others, amongst whom was Mr. PLOWMAN, the Surgeon, it was considered to have arisen from inhaling the mercurialized atmosphere; and from the preceding details, I

think there cannot remain a doubt that this opinion was the true one.

It is well known that mercury, in its native state, has often been administered in very large doses, in cases of obstinate constipation, without producing any specific effect on the system, merely removing the affection by its specific gravity. I have, however, reason to believe, from the accounts of Orfila, and others, that if the mercury was to be retained in the intestines for some time, and thus subjected to the action of the contents of the stomach and bowels, a part might become oxydated, and being conveyed into the system by means of the absorbents, would there show its specific effects.

But after the removal of the provisions, &c. at Gibraltar, many fresh cases occurred, and many relapses amongst those who had been cured out of the ship, took place on their return to duty on board, which effectually destroys the probability of this having been the cause of the succeeding ptyalism, and other morbid affections.

It only remains for me to offer my opinion, of the manner in which the system became saturated by the mercury, and this I conceive to have been effected by inhaling the mercurial vapours ; the quicksilver being then in the most perfect state of division, was readily taken up by the absorbents of the lungs, and soon showed its influence on the system generally. This idea is very much strengthened by the effect which was produced on the animals on board, already mentioned, as well as by the circumstance of a great number of men being attacked after the ship was cleared at Gibraltar, and till she arrived in a more northern latitude.

It may be considered out of place here, to give any detail of the curative means employed, I shall therefore only briefly state that sulphur, given in large quantities internally, produced no alleviation of the symptoms; on the contrary, it greatly augmented the bowel complaints, with which many of the men were affected, and brought on a most severe tenesmus; consequently, it was laid aside; applied externally, it was of no use.

The only plan which produced effectual relief was removal from the ship, with the frequent use of small doses of neutral salts and detergent gargles.

W. BURNETT.

XXVIII. *On the Astronomical Refractions.* *By* J. IVORY, *A. M. F. R. S.*

Read June 19, 1823.

1. IT was known to the ancient astronomers that there is a difference between the real and apparent places of the stars, arising from the refraction of light in its passage through the atmosphere. TYCHO BRAHE' was the first who attempted to free his observations from the effect of this irregularity. Since his time, the astronomical refraction has become more and more an object of attention, as it is found to have the greatest influence on the delicate exactness of modern observations. In the course of the last twenty years, many researches on this subject have been published by philosophers of the first note, who have applied all the resources, both of theory and practice, to overcome the difficulties which it presents. By these means our knowledge has been greatly extended; but the problem of the refractions must still be considered as the most imperfect part of modern astronomy.

The first hypothesis for bringing the astronomical refraction under a regular mode of calculation was proposed by CASSINI. He supposed that the atmosphere is a spherical shell consisting of a transparent fluid uniform in its density, which reaches to a certain height above the earth's surface. In this manner the change in the direction of the light coming from a star, is effected at the outer surface of the pellucid

medium, and it is computed by the most elementary principles of optics. This hypothesis, although extremely simple, leads to a rule for the refractions which, to a certain extent, is as accurate as any other. Perhaps it is owing to its great simplicity, that the method of Cassini seems not to have met from astronomers with the attention it deserves. Another hypothesis attributes a variable density to the atmosphere, but assumes that the rate of decrease is exactly proportional to the height ascended. This supposition is in some degree less inaccurate than that of Cassini. Most of the formulæ for the refractions that have obtained any extensive use in astronomy may be deduced from it. Kramp took a more extended view of the problem, and one less exceptionable, as approaching nearer to nature. He conducted his calculations by the real laws that regulate the density of the air, namely, the pressure and temperature. Laplace coincides with Kramp in the general view he takes of this theory; but, in treating it, he has given new proofs of that sagacity and mathematical skill, which has enabled him to accomplish so much in physical science. The table, computed by the theory of Laplace, first published in 1806, perhaps at this day gives the law of the mean refractions with greater accuracy than any other, whether founded on theory or observation.

In the hypothesis of Cassini, the atmosphere extends no higher than five miles above the earth's surface. On the supposition of a density decreasing at an equable rate, its height is limited to ten miles; but in the view of the problem taken by Kramp and Laplace, the atmosphere extends indefinitely into space. In the two first hypotheses, the horizontal refraction is considerably less than the observed

quantity; in the last, it is much greater than the truth. Now we may suppose an infinite number of atmospheres, gradually increasing in height, to be interposed between the two extreme cases; and as the horizontal refraction appears to increase with the height, there must be some intermediate case which will quadrate with observation in this respect. If we reflect that all these atmospheres will agree in giving the refractions actually observed by astronomers as far as 70° or 80° from the zenith, it is natural to think that the one which likewise coincides with nature at the horizon, will deviate but little from the truth in the intermediate 10°. At any rate we may conjecture, that the height of the atmosphere is an element in the problem that ought not to be neglected. It may be argued indeed that the infinite atmosphere considered by KRAMP and LAPLACE, will hardly be different, mathematically speaking, from one of such considerable altitude as we must suppose in the case of the earth; and that in reality, all very high atmospheres may be reckoned as forming only one case, or at least as leading to results differing from one another only by insensible shades, that may safely be neglected in practice. This observation is probably well founded; and, beyond a certain limit, it must undoubtedly be true; but in a problem of such capital importance in astronomy, the point deserves at least to be examined; more especially as it may lead to some more certain knowledge than we have yet acquired, with respect to the extent and constitution of our atmosphere.

We have no direct knowledge of the height of the atmosphere, except what is derived from the duration of the twilight, and from the great elevation at which meteors are

occasionally observed in it. From these sources we learn that the air extends forty or fifty miles above the earth's surface, and even at that altitude still continues to possess a density sufficient for refracting and reflecting the rays of light.

The authors* who have written on the height and figure of the atmosphere have likewise assigned a boundary, beyond which it cannot reach. But in this they have rather fixed a limit to the domain peculiarly belonging to the earth, than reasoned upon any distinguishing properties of the atmosphere. If we conceive a body that circulates round the earth by the force of gravitation in the time of a diurnal revolution, the path which it describes will mark the limit where the centrifugal force arising from the rotatory motion of the earth, will just balance the opposite centripetal force. Therefore any body that participates of the rotatory motion common to all, if placed beyond the boundary we have mentioned, would continually recede from the earth, and would be lost in the immensity of space; if placed within the same boundary, it would fall to the common centre. The radius of the orbit described by the revolving body is about 25,000 miles, or something more than three diameters of the terrestrial globe. Now the air surrounding the earth cannot reach so far; for if it did, it would be continually dissipated; a supposition which is extremely improbable, since we are acquainted with no source from which a constant waste of so necessary a fluid might be supplied.

But if we would acquire more correct notions as to the height of the atmosphere, we must consider more closely the

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* D'ALEMBERT, Opus. Tom. 6. LAPLACE, Mec. Celeste, Liv. 3. Cap. 7.

principles on which it must depend. Conceive a cylinder of air extending indefinitely in a vertical direction, and let it be divided into equal parts of a moderate length, so that the density of every division may be considered as uniform: then, if we abstract from the diminution of gravity and the increase of the centrifugal force, which are inconsiderable within 200 or 300 miles of the earth's surface, the weight of the air in every portion of the cylinder will be proportional to its density. Now, if we admit that the elastic force is likewise proportional to the density, as it would be in an atmosphere of uniform temperature, it will follow, that the weights of the several divisions of the cylinder will vary in the same proportion as their elasticities. But in the lowest part of the cylinder, the weight of the small quantity of air contained in one division, is incomparably less than its elastic force, which is an equipoise to the whole atmosphere: and the same thing will therefore be true of every portion of the cylinder, however high it is placed. Hence an atmosphere constituted as we have supposed, must necessarily be infinite in its extent. For if it were finite, since there is no pressure at the surface, the weight of a volume of air situated there would be in equilibrium with its elastic force, whereas it has been proved that the former is always an inconsiderable part of the latter.

But in the foregoing reasoning, a cause is neglected which diminishes the elasticity of the air as we ascend above the earth's surface, without affecting the force of gravity in any degree. In the higher parts of the atmosphere a continually increasing degree of cold is found to prevail. Now, the effect of cold is to contract all bodies in their dimensions;

and therefore, by the operation of this cause, as we ascend in the atmosphere, the expansive force of a given volume of air is constantly diminished and brought nearer to an equality with its weight. To estimate this effect with greater precision, let p', z', t', denote the barometric pressure, the density, and the temperature by the centigrade thermometer, at the earth's surface; and let the same letters, without the accent, denote the same things at any height x; then, if $\beta = \frac{3}{800}$, the expansion for one centigrade degree, the known laws that obtain in the expansion of elastic fluids, will lead to this formula, viz.

$$\frac{p}{p'} = \frac{1 + \beta t}{1 + \beta t'} \times \frac{z}{z'}$$

Now here $\frac{p}{p'}$ is the measure of the elastic force at the height x in parts of the same force at the surface; and we see that it depends on the temperature as well as on the relative density $\frac{z}{z'}$. At the earth's surface the quantity $\frac{1 + \beta t}{1 + \beta t'}$ is equal to unit, but it continually decreases as the temperature becomes less in ascending. We cannot conceive that it will become negative, nor can we set any bounds to its approach to zero. But when $\frac{1 + \beta t}{1 + \beta t'}$ is evanescent, or when $t = - 266°$, the elastic force of the air will cease, and gravity will stop the farther dilatation of the atmosphere. This reasoning is independent of the law of the densities; and it proves both that the atmosphere may be finite in its extent; and that it may have a finite density at its upper surface. But it may be objected, that the effect of temperature on the air's elasticity has been verified only to a certain extent; and that in the case of air of great rarity, and subjected to extreme

degrees of cold, the law of dilatation and contraction may be very different from what it has been observed to be in the limited range of our experiments. This observation is probably well founded, but it will not destroy the force of what has been advanced. We know that air always gives out heat when it is compressed into a less volume, and absorbs heat when it expands. As long therefore as that fluid retains its elasticity, so long, we must conclude, will temperature continue to modify the changes of bulk which that force produces. The law of dilatation and contraction may no doubt undergo some change in different circumstances, but every expansion must be productive of cold, and every new degree of cold must diminish the elastic force of a given volume of air. Gravity continuing to act with nearly the same energy, while the elastic force of the air is continually diminished, these two forces will at length become equivalent, and will counterbalance one another, which is all that is necessary for imposing a limit to the extent of the atmosphere. We have proved that air, if it were confined by the action of gravity alone, would extend indefinitely into space; and it is not unreasonable to consider the effect of temperature as a contrivance for securely attaching to the terrestrial globe a fluid so necessary in every point of view to the economy of nature.

Since it is found that all elastic fluids follow the same laws in regard to heat and pressure, the foregoing reasoning is equally true, whether we conceive the atmosphere as composed of one homogeneous fluid, or as a collection of many elastic gases and vapours, however much they may differ from one another in specific gravity.

It may even be possible to form some reasonable conjecture

as to the actual height of the finite atmosphere. Gay Lussac ascended in a balloon to the altitude of 3816 English fathoms, or nearly $4\frac{1}{4}$ miles above the level of the Seine at Paris; the proportion of the heights of the barometer in the balloon and at the surface of the earth being 0.467 nearly, which is therefore the relative elasticity of the air. The temperatures, as observed at the extremities of the elevation, were 30°.8, and —9°.5 on the centigrade scale; and if we increase 0.467 to what it would have been, had the temperature remained unchanged during the ascent, we shall find 0.500, which is the density of the air at the height ascended in parts of the density at the surface of the earth. Thus, in the decreasing scale of elasticities, the diminution is from 1 to 0.467; but, in the decreasing scale of densities, it is only from 1 to 0.500. The quantities of the one scale continually fall behind those of the other at a rate that must bring them to zero, whatever be the gradation of the latter. If we divide 3816 fathoms, the whole height ascended, by 40°.3, the difference of temperature, the elevation for depressing the thermometer one degree will come out equal to 95 fathoms: and if we suppose that the same rate prevails in all parts of the atmosphere, the whole height will be 266 × 95 fathoms, or nearly 29 miles. The observations of the twilight show that this is less than the true altitude; and hence we must infer, that the thermometer falls at a slower rate in the higher, than in the lower, parts of the atmosphere. But, taking the observed rate of 95 fathoms for the first 40 degrees, and allowing, on an average, a double, or even a triple, elevation for the remaining 226°, we shall still find that the atmosphere will extend only to a moderate height above the earth's surface.

2. The first investigation that presents itself in the problem of the refractions, is to find the velocity of light at any given height in the atmosphere. The only physical principle wanted for this purpose, is the refractive power of air according to its density. Hauksbee first determined by experiment, that air refracts light in proportion to its density; and this result has been confirmed by succeeding philosophers. There is even good reason to think that the conclusion of Hauksbee is not materially affected by the variable quantities of aqueous vapour contained in the atmosphere at different times. Admitting, then, the principle we have mentioned, we must conceive that the light coming from the sun, or from a star, moves in vacuo with a uniform velocity till it reaches the atmosphere. It is there deflected from its course by the spherical and concentric shells of air it meets with, each of which acts upon it with a force perpendicular to its surface, and directed to the centre of the earth. Now, as all the light enters the atmosphere with the same velocity; and as the deflecting forces are of the same intensity at the same distance from the common centre to which they tend; it follows, that the new velocities acquired by the action of the forces, will be independent of the direction of the light's motion, and will be the same at the same distance above the earth's surface. Let a denote the radius of the earth; x, any height in the atmosphere; and ρ, the density of the air at that height: conceive also a shell of air having the thickness δx, and the increased density $\varrho + \delta\rho$; then we shall have to consider the relation between the velocities of light when it passes out of a medium having the density ϱ, into another medium having the density $\varrho + \delta\rho$; or, since the density common to both

media has no effect in altering the velocity, we may consider the more simple case, when light passes out of a vacuum into a medium possessed of the density $\delta\varrho$. It is to be observed that the forces, with which matter acts on the rays of light, extend to distances that are imperceptible to our senses, and incapable of being measured ; and, on this account, what has been said is modified in no respect by the thinness of the shell of air. However small δx, the thickness of the shell is supposed to be, it may still be considered as infinitely great in comparison of the range of the corpuscular force with which the light is refracted by the air. If we now put v for the velocity with which the light enters the shell of air, and express by an equation the physical principle already mentioned, namely, that the refractive power of air is proportional to its density, we shall get,

$$(v+\delta v)^2-v^2=\mathrm{K}\,\delta\rho,\text{*}$$

K expressing a constant coefficient to be determined by experiment. And, because v and ϱ are functions of the same variable quantity x, the foregoing equation may be translated into the language of the differential calculus, in which case it will become,

$$d\cdot v^2=\mathrm{K}\times d\varrho:$$

and, by integrating,

$$v^2=1+\mathrm{K}\varrho,$$

$$v=\sqrt{1+\mathrm{K}\varrho};$$

unit representing the primitive velocity of the light in vacuo.

Let us next consider the trajectory described by the light in its passage through the atmosphere. Conceive two perpendiculars to be let fall upon the tangents drawn to the

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* NEWTON's Optics, Book 2, Part 3, Prop. X.

trajectory from the points where the light enters into, and passes out of, the spherical shell of air; then, if y represent the latter of these two lines, $y+dy$ will be equal to the former. The distance of the intersection of the two tangents from the centre of the earth being $a+x$, $\sqrt{(a+x)^2-y^2}$ will be the distance of the perpendiculars from the same intersection; and, on a circle described with the radius $\sqrt{(a+x)^2-y^2}$, dy is the arc that subtends the small angle contained by the two tangents; wherefore, if dr denote the measure of the small angle, we shall have

$$dy = dr \times \sqrt{(a+x)^2-y^2};$$

and,

$$dr = \frac{dy}{\sqrt{(a+x)^2-y^2}}.$$

Again; because the light is continually deflected in a direction tending to the centre of the earth, equal areas will be described round that centre in equal times by the motion in the trajectory; but the areas described in equal times are proportional to the velocities multiplied by the perpendiculars falling upon the tangents from the centre of forces: wherefore, the product $v \times y$ will have always the same magnitude at every point of the curve. Let v' be the velocity of the light at the surface of the earth, or at the point where the trajectory enters the eye of the observer; and put y' for the perpendicular upon the tangent drawn from the same point of the curve: then,

$$v \times y = v' \times y'$$

$$y = \frac{v'}{v} \times y'.$$

Suppose also that θ is the apparent zenith distance of the star, or the angle which the last-mentioned tangent makes with

the vertical of the observer, then $y = a\,\mathrm{Sin.}\,\theta$: again, if ϱ' denote the density of the air at the surface of the earth, we get, by the formula before investigated, $\upsilon' = \sqrt{1 + \mathrm{K}\rho'}$, $\upsilon = \sqrt{1 + \mathrm{K}\rho}$: wherefore

$$y = \frac{\upsilon'}{\upsilon} \times y' = a\,\mathrm{Sin.}\,\theta \times \sqrt{\frac{1 + \mathrm{K}\rho'}{1 + \mathrm{K}\rho}};$$

and, if we put $\omega = 1 - \frac{\varrho}{\rho'}$,

$$y = a\,\mathrm{Sin.}\,\theta \times \sqrt{\frac{1 + \mathrm{K}\varrho'}{1 + \mathrm{K}\rho' - \mathrm{K}\rho'\omega}};$$

and finally,

$$\alpha = \frac{\frac{1}{2}\mathrm{K}\varrho'}{1 + \mathrm{K}\rho'},$$

$$y = \frac{a\,\mathrm{Sin.}\,\theta}{\sqrt{1 - 2\alpha\omega}}.$$

Let this value of y be substituted in the expression of dr already obtained, then

$$dr = \frac{\alpha\,d\omega}{1 - 2\alpha\omega} \times \frac{\mathrm{Sin.}\,\theta}{\sqrt{\left(1 + \frac{x}{a}\right)^2 (1 - 2\alpha\omega) - \mathrm{Sin.}^2\,\theta}}.$$

Whatever opinion we adopt concerning the height of the atmosphere, $\frac{x}{a}$ may be considered as a very small quantity. For, in every hypothesis, the density of the air is attenuated so fast in ascending, that it may be taken as evanescent at an altitude extremely small in proportion to the earth's radius. The quantity α is also a very small fraction; and hence it will be sufficiently accurate if, in the foregoing expression, we put

$$\left(1 + \frac{x}{a}\right)^2 (1 - 2\alpha\omega) = 1 + 2\,\frac{x}{a} - 2\alpha\omega;$$

by which means we obtain,

$$dr = \frac{\alpha\,d\omega}{1 - 2\alpha\omega} \times \frac{\mathrm{Sin.}\,\theta}{\sqrt{\mathrm{Cos.}^2\,\theta + 2\,\frac{x}{a} - 2\alpha\omega}}$$

In this formula ω and r increase in ascending above the origin

of the curve placed at the eye of the observer; and, at any determinate height, r is the angle contained by two tangents drawn from the extremities of the intercepted arc; or it is the sum of the angles which the two tangents make with the chord of the arc. When the curve is continued to the boundary of the atmosphere, or at least so high that the air has no longer power to deflect the light from its rectilineal course, the chord may be considered as parallel to the tangent at the remote extremity; and then r is the astronomical refraction. The formula is perfectly general, and will apply in all hypotheses of density, since no particular relation is established between the variable quantities ω and x.

3. But there are relations between the pressure of the air and its density and temperature, which must be attended to in the solution of this problem. Let p' and τ' denote the barometric pressure and the temperature on the centigrade scale at the surface of the earth, and put the same letters, without the accent, for the same things at the height x: then, if $\beta = \frac{3}{800}$, the expansion for one degree of the thermometer, we shall have

$$\frac{p}{p'} = \frac{1 + \beta\tau}{1 + \beta\tau'} \times \frac{\rho}{\rho'}.$$

In order to prove the truth of this formula, we may suppose a volume of air to be inclosed in a manometer, the pressure being p', the density ρ', and the temperature τ': then, if the pressure be changed to p, the temperature remaining the same, the density will become,

$$\frac{p}{p'} \times \rho':$$

and, if the temperature be now likewise changed to τ, the new density will be equal to

$$\frac{p}{p'} \times \frac{1+\beta\tau'}{1+\beta\tau} \times \rho' :$$

but we have put ρ for the density when the pressure is p and the temperature τ; wherefore,

$$\frac{p}{p'} \times \frac{1+\beta\tau'}{1+\beta\tau} \times \rho' = \varrho,$$

which is equivalent to the foregoing formula.

From what has just been proved we get,

$$\frac{p'}{(1+\beta\tau')\rho'} = \frac{p}{(1+\beta\tau)\rho};$$

which shows that the quotient of the pressure divided by the density reduced to the fixed temperature zero, has always the same value. Therefore if l denote this constant value, we shall have,

$$p = l \times (1+\beta\tau) \times \rho;$$

and l is a quantity to be determined by experiment.

Suppose now that a tube or cylinder of air extends from the surface of the earth to the top of the atmosphere; then the barometric column p, will be equal to the pressure of all the air in the cylinder above the height x. Let the barometer be lowered down through the small space dx; the mercury will rise a small height dp; and we shall have

$$dp = -dx \times \rho,$$

an equation which merely expresses that the small column of mercury dp is equivalent to the weight of the column of air having its length equal to dx, and its density to ρ. Divide the left side of the equation by p', and the right side by the equivalent quantity $l \times (1+\beta\tau') \times \rho'$; then,

$$\frac{dp}{p'} = -\frac{dx}{l \times (1+\beta\tau')} \times \frac{\rho}{\rho'};$$

and by integrating,

$$\frac{p}{p'} = \int \frac{-dx}{l(1+\beta\tau')} \times \frac{\rho}{\rho'}.$$

In order to simplify, I shall now write P for the relative pressure $\frac{p}{p'}$, and likewise put $s = \frac{x}{l \times (1 + \beta\tau')}$; then, observing that $\frac{\rho}{\rho'} = 1 - \omega$, what has now been investigated will be expressed by these equations, viz.

$$\left.\begin{aligned} P &= \int - ds\,(1 - \omega) \\ P &= \frac{1 + \beta\tau}{1 + \beta\tau'} \times (1 - \omega) \\ s &= \frac{x}{(1 + \beta\tau')}. \end{aligned}\right\} (A)$$

The quantity $\frac{1 + \beta\tau}{1 + \beta\tau'}$, is equal to the proportion of the relative elasticity of the air to its relative density; and it may depend upon the moisture diffused in the atmosphere, as well as upon the temperature. Whatever be the true form of this function, it must be evanescent at the boundary of the atmosphere. The reason of this will readily appear, if we consider first, that, at the surface of the earth, the elasticity of a given volume of air is incomparably greater than its weight; and, secondly, that in a finite atmosphere, there must be an equality between the same two forces at the upper surface. With regard to the density, we may form two suppositions; it may either be evanescent at the top of the atmosphere, or it may have some very small finite value. But in reality we know that, in ascending, the density of the air decreases with considerable rapidity; so that if it do not decrease so as to be absolutely evanescent, it must finally become so small, that we may safely consider it as equal to zero.

To the equations already investigated we must add another, which is requisite to the solution of this Problem, although it has been universally neglected. By equating the

two foregoing values of p, we shall obtain

$$\frac{1+\beta\tau}{1+\beta\tau'} = \frac{\int - ds\,(1-\omega)}{1-\omega};$$

and by taking the fluxions,

$$\frac{d.\left(\frac{1+\beta\tau}{1+\beta\tau'}\right)}{ds} = -1 + \frac{d\omega}{ds}.\frac{\int - ds\,(1-\omega)}{(1-\omega)^2}:$$

but $ds = \frac{dx}{l(1+\beta\tau')}$; therefore, observing that τ decreases when x increases,

$$-\beta l \times \frac{d\tau}{dx} = -1 + \frac{d\omega}{ds}.\frac{\int - ds\,(1-\omega)}{(1-\omega)^2};$$

now, at the surface of the earth, $P = \int - ds\,(1-\omega) = 1$: wherefore,

$$\frac{d\omega}{ds}\,(\text{when } s = 0) = 1 - \beta l \times \frac{d\tau}{dx}.$$

Suppose that μ represents the height through which the thermometer must be carried at the surface of the earth, in order to depress the mercury one degree; it is obvious, that $\frac{1}{\mu}$ is the numerical value of $\frac{d\tau}{dx}$: wherefore,

$$\frac{d\omega}{ds}\,(\text{making } s = 0) = 1 - \frac{\beta l}{\mu}. \quad \text{(B)}$$

The quantity $\frac{d\omega}{ds}$ is derived from the function of the height that represents the decrease of density. It appears that the value of it at the surface of the earth depends upon μ; and terrestrial observations show that this quantity is subject to great irregularities, which are not well understood. It is found that the refractions near the horizon are liable to variations equally irregular and unknown. There can be little doubt that both these effects are produced by the same causes, which disturb the gradation of heat, and the arrangement of the strata of air near the earth's surface.

It will now be necessary to resume the former value of dr,

and in it to substitute s for x. Now, $s = \frac{x}{l(1+\beta\tau')} = \frac{x}{a} \times \frac{a}{l(1+\beta\tau')}$: if therefore we put $i = \frac{l(1+\beta\tau')}{a}$, we shall get $is = \frac{x}{a}$, and

$$dr = \frac{\alpha\, d\omega}{1 - 2\alpha\omega} \times \frac{\text{Sin.}\,\theta}{\sqrt{\text{Cos.}^2\theta + 2is - 2\alpha\omega}};$$

and, by expanding $\frac{1}{1-2\alpha\omega}$,

$$\begin{aligned} dr = {} & \alpha\,\text{Sin.}\,\theta \times \frac{d\omega}{\sqrt{\text{Cos.}^2\theta + 2is - 2\alpha\omega}} \\ & + \alpha^2\,\text{Sin.}\,\theta \times \frac{2\omega\, d\omega}{\sqrt{\text{Cos.}^2\theta + 2is - 2\alpha\omega}} \\ & + \&c. \end{aligned}$$

The second term of this expansion has to the first a less proportion than that of α to 1, while ω increases from 0 to $\frac{1}{2}$; and a greater proportion, while ω increases from $\frac{1}{2}$ to 1: and hence, on account of the smallness of α, we may combine both terms in one, viz.

$$dr = \alpha(1+\alpha)\,\text{Sin.}\,\theta \times \frac{d\omega}{\sqrt{\text{Cos.}^2\theta + 2is - 2\alpha\omega}}. \quad \text{(C)}$$

4. In order to appreciate justly the several formulæ on which this theory depends, it is necessary to know the values of the quantities that must be found from observation. Of these, the coefficient α has been determined both astronomically, and by direct experiments on the refractive power of the air. From the comparison of a great number of astronomical observations, De Lambre found $K\rho' = \cdot 000588094$, at the temperature of melting ice, and the mercury in the barometer standing at 29.921 English inches. In the same circumstances, M. M. Biot and Arago, by very accurate experiments on the refraction of air inclosed in a prism, found .000588768 for the value of the same quantity. Adopting

the number of DE LAMBRE, which is that employed in the calculation of the French tables of refraction, we get

$$\alpha = \frac{\frac{1}{2}K\rho'}{1 + K\rho'} = .000293876;$$

and, by reducing to the mean temperature of 10° on the centigrade scale, or 50° of FAHRENHEIT, and to the standard barometer 30 English inches, we finally obtain

$$\alpha = .0002835,$$

$$\text{Log.} — 4.4525531.$$

From a numerous set of observations Dr. BRINKLEY has deduced a value somewhat less than the preceding; and hence it appears, that there is still some small degree of uncertainty in the determination of this coefficient. It is to be expected that the unequal mixture of moisture, by altering the density of the air, will produce variations in the value of α. But it has been determined that, when a quantity of aqueous vapour is added to a volume of air, the density is diminished nearly in the same proportion that the refractive power of the vapour is greater than the refractive power of the air. A compensation is thus effected; and the mixed medium is hardly different from dry air of the like density in its action on light.

The value of l must be found by means of the formula

$$p = l \times (1 + \beta\tau)\varrho.$$

Here we must conceive that ϱ is measured in parts of the density of mercury; and, as $(1 + \beta\tau)\varrho$ is the density of the air reduced to the fixed temperature zero, the equation merely expresses that the density of air is proportional to the pressure when the temperature remains unchanged. Now, M. M. BIOT and ARAGO have found that the specific gravity of air under the pressure of 0.76 metres, and at the temperature of

melting ice, is to the specific gravity of mercury at the same temperature, as 1 to 10467: hence we have $p = 0.76, \rho = \frac{1}{10467}$, $\tau = o$; and, by the substitution of these numbers, we get,

$$l = 10467 \times 0.76 \text{ metres},$$

or, in English fathoms,

$$l = 4349.8.$$

This is the length of l at the temperature of melting ice; but, if the temperature be changed, it will vary directly as the volume of the air, and inversely as that of the mercury. If now we take for the radius of the earth $(= a)$, a mean between half the polar axis and the radius of the equator, and reduce the foregoing value of l to the mean temperature of 50° of Fahrenheit, we shall get,

$$\left.\begin{array}{l} l = 4504.8 \\ a = 3481280 \end{array}\right\} \text{fathoms}$$

$$\frac{l}{a} = .001294; \text{ Log.} — 3.1119343.$$

The value of μ, or the height through which the thermometer must be carried at the earth's surface, in order to depress the mercury one degree, has not been determined with much certainty or exactness. The greatest irregularity is found to prevail, in regard to this element, in observations made on different heights and at different times. This is, no doubt, to be attributed in part to local peculiarities affecting the thermometer. The most accurate way of determining this element would be by means of observations made in balloons elevated to moderate heights. Ramond, from 38 barometrical measurements, makes the mean depression for one centesimal degree equal to 164.7 metres, or 90 fathoms; Humboldt found 161 metres, or 88 fathoms; and the ascent

of GAY LUSSAC gives 174 metres, or 95 fathoms. With these several values we shall find $\frac{\beta l}{\mu}$ equal to 0.188, 0.192, and 0.177 respectively; and we may adopt $\frac{1}{5}$ as an approximation. Thus,

$$\frac{\beta l}{\mu} = \frac{1}{5}$$

$$\frac{d\omega}{ds}\,(\text{making } s = o) = 1 - \frac{\beta l}{\mu} = \frac{4}{5}.$$

5. In one particular case of this problem we are possessed of many skilful observations made in the course of the trigonometrical surveys of England and France. We allude to the terrestrial refraction, which regards that part of the trajectory described by the light in its passage from a terrestrial object to the eye of the observer. As this case is immediately deduced from the equations that have been investigated, the comparison of the result with observations may, in some degree, instruct us how far the theory will agree with nature.

We have found this equation, viz.

$$\frac{d\omega}{ds}\,(\text{making } s = o) = \frac{4}{5};$$

which being accurately true at the surface of the earth, it may, without sensible error, be extended to a small height above the surface. In the case of the terrestrial refraction we thus have,

$$\omega = \frac{4}{5}s;$$

and, if this value be substituted in the expression of dr, we get,

$$dr = \alpha\,\text{Sin.}\,\theta \times \frac{\frac{4}{5}ds}{\sqrt{\text{Cos.}^2\theta + 2\left(i - \frac{4}{5}\alpha\right)s}}.$$

By integrating

$$r = \frac{4}{5} \times \frac{\alpha\,\text{Sin.}\,\theta}{i - \frac{4}{5}\alpha} \times \left\{\sqrt{\text{Cos.}^2\theta + 2\left(i - \frac{4}{5}\alpha\right)s} - \text{Cos.}\,\theta\right\}:$$

and, if the observed object be just 90° from the zenith, then

$$r = \frac{4\alpha}{5} \times \frac{\sqrt{2s}}{\sqrt{i - \frac{4}{5}\alpha}}.$$

Now $s = \frac{x}{l(1 + \beta\tau')}$; and neglecting the effect of temperature, $s = \frac{x}{l}$. Let ν be the angle at the earth's centre contained by lines drawn to the observer and the object; then, x being the height between the surface of the earth and the tangent to that surface drawn from the place of the observer, we have $a.\nu^2 = 2x$; and hence $2s = \frac{2x}{l} = \frac{a}{l}\nu^2 = \frac{\nu^2}{i}$: consequently,

$$\frac{r}{2} = \frac{2}{5} \times \frac{\alpha}{i} \cdot \frac{\nu}{\sqrt{1 - \frac{4}{5} \cdot \frac{\alpha}{i}}};$$

and, in numbers,

$$\frac{r}{2} = \frac{\nu}{10.36}.$$

Now r is the sum of the angles which the tangents, drawn from the extremities of the arc intercepted between the observer and the object, make with the chord of the arc; and, as the curvature will vary but little in a small extent, the two angles may be considered as equal, and $\frac{r}{2}$ will be the refraction at the eye of the observer. When the terrestrial refraction, as found by actual observation, is compared with the angle at the earth's centre, it is very irregular, varying from $\frac{1}{2}$ to $\frac{1}{24}$. In a case, where such excessive irregularities occur, no great confidence can be placed in a mean, even of a great number of observations; more especially as local peculiarities have so much effect, that the mean at one place does not agree with the mean at another. In the English Survey, $\frac{1}{10}$ is allowed for the terrestrial

refraction; the French mathematicians make it equal to $\frac{1}{12}$; and the result found above falls between these limits.

6. In the expression of the refraction (C), the quantities i and α are very small fractions; and $\cos.^2\theta$ varies from 1 to 0 as the zenith distance increases from 0 to 90°. For a considerable extent from the zenith $\cos.^2\theta$ will greatly exceed i and α; and so long as this is the case, we may find the value of r by expanding the radical quantity in a series. Proceeding in this manner, and retaining only the two first terms of the expansion, we shall get

$$dr = \alpha(1+\alpha)\text{Tan.}\,\theta \times \left\{d\omega - \frac{is\,d\omega - \alpha\omega\,d\omega}{\text{Cor.}^2\theta}\right\}:$$

and, by integrating from $\omega = 0$ to $\omega = 1$,

$$r = \alpha\,\text{Tan.}\,\theta \times \left\{1 + \alpha - \frac{i\int s\,d\omega - \frac{1}{2}\alpha}{\text{Cor.}^2\theta}\right\},$$

the terms multiplied by α, $i\alpha$, α^2 being alone retained. Now

$$s(1-\omega) = \int ds(1-\omega) - \int s\,d\omega;$$

and because $s(1-\omega) = 0$, both when $\omega = 0$ and $\omega = 1$, if we take the whole integrals between these limits, we get

$$\int s\,d\omega = \int ds(1-\omega).$$

But $\int ds(1-\omega)$ between the limits $\omega = 0$ and $\omega = 1$ has the same value that $\int -ds(1-\omega)$ has, between the limits $\omega = 1$ and $\omega = 0$; and this last integral is equal to the whole pressure, or to unit: wherefore

$$\int s\,d\omega = 1;$$

and, by substitution,

$$r = \alpha\,\text{Tan.}\,\theta \times \left\{1 + \alpha - \frac{i - \frac{1}{2}\alpha}{\text{Cos.}^2\theta}\right\}.$$

By means of this formula, which was first found by LAPLACE, the French tables of refraction are computed as far as 74° from the zenith. The quantities i and α, depending only upon

the temperature and pressure of the lowest stratum of air and upon the radius of the earth, the formula involves no hypothesis concerning the gradation of heat or density. But if the expansion of the expression of the refraction be extended to more terms, we meet with quantities that cannot be integrated without supposing a relation between s and ω, that is, without introducing a supposition respecting the constitution of the atmosphere.

The ultimate deviation of the light of a star from its primitive direction depends upon the augmentation of the velocity which the light acquires in its passage through the atmosphere, and likewise upon the different obliquities with which it crosses the several strata of air. Now, the first of these two things is the same for all stars and for all constitutions of the atmosphere; for it is the same when the density of the lowest stratum of air continues the same. But the second is different for stars that are differently placed with regard to the zenith: and it varies also with the densities of the strata that compose the atmosphere. It is therefore certain that the formula of Laplace is rigorously exact in no case whatever. But when a star is near the zenith, the variations in the obliquity of the light in passing through the several strata of air, are inconsiderable; and the formula will be nearly true. However, there is always some error, which accumulates as the zenith distance increases, and will at length become sensible. Delambre tells us that in comparing the observations of different days, he found errors arising from refraction that amounted to 6″ or 7″ at 75° from the zenith;* and the observations of a very accurate astronomer

* Astron. Vol. 1. p. 320.

show that similar inequalities are perceptible much nearer the zenith.* Now these inequalities do not arise from any thing imperfect in the manner of observing; they are undoubtedly produced by alterations in the remote parts of the atmosphere, which do not affect the barometer or the thermometer placed at the Observatory. It appears, therefore, that the peculiar constitution of the atmosphere has a perceptible influence on the refraction at 75° from the zenith; and when Laplace's formula is made to extend to 74°, it is carried to its utmost limit.

However mutable we may suppose the condition of the atmosphere to be, there must be a mean state equally removed from the opposite extremes. Now, a table of refractions that should have this mean state of the atmosphere for its basis, would be the most advantageous of any. For although, with respect to single observations, the errors of such a table might be as great as in some other hypotheses, yet, in a numerous set of observations made at different times, so as to embrace all the usual changes, the inequalities of an opposite kind would counterbalance one another. But, to a certain distance from the zenith, Laplace's formula is sufficiently exact for practical purposes; and it has the advantage of taking away the necessity of having recourse to precarious suppositions respecting the constitution of the atmosphere.

As the formula we are considering contains nothing except what is common to every atmosphere, it must be deducible from the hypothesis of Cassini; and it may be worth while to establish this point by a strict investigation. Cassini supposed that the earth is surrounded by a pellucid spherical

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* Dr. Brinkley's Paper, Philosophical Transactions, 1821, p. 342.

shell of uniform density, reaching to a certain altitude, and possessed of the same weight with the real atmosphere. The height of this homogeneous stratum of air will therefore be equal to the quantity l, before investigated. Suppose that the light of a star is refracted at the upper surface of this atmosphere in a straight line directed to the eye of the observer, and making an angle θ with the vertical line; then the perpendicular let fall upon the refracted ray from the earth's centre will be equal to $a \operatorname{Sin.} \theta$: and, in a right-angled triangle, of which $a + l$ is the hypothenuse, $a \operatorname{Sin.} \theta$ one side, and φ the angle at the top of the atmosphere opposite to that side, we have,

$$\operatorname{Sin.} \varphi = \frac{a \operatorname{Sin.} \theta}{a + l} = \frac{\operatorname{Sin.} \theta}{1 + \frac{l}{a}} = \frac{\operatorname{Sin.} \theta}{1 + i},$$

$$\operatorname{Cos.} \varphi = \frac{\sqrt{\operatorname{Cos.}^2 \theta + 2i + i^2}}{1 + i},$$

$$\operatorname{Tan.} \varphi = \frac{\operatorname{Sin.} \theta}{\sqrt{\operatorname{Cos.}^2 \theta + 2i + i^2}}.$$

It is manifest that φ is the angle of refraction; and if r be the refraction, or the angle between the incident and refracted light, $\varphi + r$ will be the angle of incidence: and $\operatorname{Sin.} (\varphi + r)$ will be to $\operatorname{Sin.} \varphi$, as the velocity of the light in air to the velocity in vacuo, that is, as $\sqrt{1 + \mathrm{K}\rho'}$ to 1, or as $\frac{1}{\sqrt{1 - 2\alpha}}$ to 1: wherefore,

$$\operatorname{Sin.} (\varphi + r) = \frac{\operatorname{Sin.} \varphi}{\sqrt{1 - 2\alpha}}$$

$$\operatorname{Cos.} (\varphi + r) = \frac{\sqrt{\operatorname{Cos.}^2 \varphi - 2\alpha}}{\sqrt{1 - 2\alpha}}.$$

But,

$$\operatorname{Sin.} r = \operatorname{Sin.} (\varphi + r) \operatorname{Cos.} \varphi - \operatorname{Cos.} (\varphi + r) \operatorname{Sin.} \varphi;$$

therefore,

$$\text{Sin.}\, r = \frac{\text{Sin.}\, \varphi \,\text{Cos.}\, \varphi - \text{Sin.}\, \varphi \sqrt{\text{Cos.}^2 \varphi - 2\alpha}}{\sqrt{1-2\alpha}}.$$

Now $r = \text{Sin.}\, r + \frac{1}{6} \text{Sin.}^3 r +$ &c.; and, at the horizon where r is greatest, all the terms of the series after the first will not amount to $\frac{1}{20}$ of a second: thus $r = \text{Sin.}\, r$, and

$$r = \frac{\text{Sin.}\, \varphi \,\text{Cos.}\, \varphi - \text{Sin.}\, \varphi \sqrt{\text{Cos.}^2 \varphi - 2\alpha}}{\sqrt{1-2\alpha}};$$

by expanding the radical quantity in the numerator,

$$r = \frac{\text{Tan.}\, \varphi}{\sqrt{1-2\alpha}} \times \left\{ \alpha + \frac{1}{2} \cdot \frac{\alpha^2}{\text{Cos.}^2 \varphi} + \frac{1}{2} \cdot \frac{\alpha^3}{\text{Cos.}^4 \varphi} + \frac{5}{8} \cdot \frac{\alpha^4}{\text{Cos.}^6 \varphi} + \&\text{c.} \right\}.$$

When $\text{Cos.}\, \theta = o$, $\text{Tan.}\, \varphi = \frac{1}{\sqrt{2i}}$, and $\frac{1}{\text{Cos.}^2 \varphi} = \frac{1}{2i}$;

and hence, even in this extreme case, the term last set down of the foregoing series, and all the following terms, may be rejected; therefore, because $\frac{1}{\text{Cos.}^2 \varphi} = 1 + \text{Tan.}^2 \varphi$, we have

$$r = \frac{\alpha + \frac{1}{2} \alpha^2}{\sqrt{1-2\alpha}} \text{Tan.}\, \varphi + \frac{\frac{1}{2} \alpha^2 + \alpha^3}{\sqrt{1-2\alpha}} \text{Tan.}^3 \varphi + \frac{\frac{1}{2} \alpha^3}{\sqrt{1-2\alpha}} \text{Tan.}^5 \varphi;$$

and farther, by rejecting the very small quantities $\alpha^3 \,\text{Tan.}\, \varphi$, $\alpha^3 \,\text{Tan.}^3 \varphi$, $\alpha^4 \,\text{Tan.}^5 \varphi$, &c. we obtain, with sufficient accuracy,

$$r = (\alpha + \tfrac{3}{2} \alpha^2) \text{Tan.}\, \varphi + \frac{\alpha^2}{2} \text{Tan.}^3 \varphi + \frac{\alpha^3}{2} \text{Tan.}^5 \varphi;$$

and finally, by substituting the value of Tan. φ,

$$\begin{aligned} r = {} & \frac{(\alpha + \frac{3}{2} \alpha^2)\, \text{Sin.}\, \theta}{\left\{ \text{Cos.}^2 \theta + 2i + i^2 \right\}^{\frac{1}{2}}} \\ & + \frac{1}{2} \cdot \frac{\alpha^2 \,\text{Sin.}^3 \theta}{\left\{ \text{Cos.}^2 \theta + 2i + i^2 \right\}^{\frac{3}{2}}} \\ & + \frac{1}{2} \cdot \frac{\alpha^3 \,\text{Sin.}^5 \theta}{\left\{ \text{Cos.}^2 \theta + 2i + i^2 \right\}^{\frac{5}{2}}}. \end{aligned}$$

If we put $\text{Sin.}\, \theta = 1$, $\text{Cos.}\, \theta = o$, we shall obtain the horizontal refraction in the hypothesis of CASSINI, viz.

$$r = \frac{\alpha}{\sqrt{2i}} \times \left\{ 1 + \frac{3}{2} \alpha + \frac{1}{4} \cdot \frac{\alpha}{i} + \frac{1}{8} \cdot \frac{\alpha^2}{i^2} \right\}.$$

To find the refractions near the zenith, we must develope the radical quantities and retain, as formerly, such terms only as are multiplied by α, αi, α^2: in this manner we get,

$$r = \alpha \,\text{Tan.}\, \theta \cdot \left\{1 + \tfrac{3}{2}\alpha - \frac{i}{\text{Cos.}^2\theta} + \tfrac{\alpha}{2}\,\text{Tan.}^2\theta\right\};$$

or,

$$r = \alpha \,\text{Tan.}\, \theta \times \left\{1 + \alpha - \frac{i - \frac{1}{2}\alpha}{\text{Cos.}^2\theta}\right\},$$

and this is no other than Laplace's formula, which is thus deducible from the most simple, as well as the most complicated, hypothesis.

The same formula may be thus written, viz.

$$r = \alpha(1+\alpha) \cdot \left\{\text{Tan.}\,\theta - \frac{i - \frac{1}{2}\alpha}{\alpha(1+\alpha)^2} \times \alpha(1+\alpha)\,\text{Tan.}\,\theta \times \frac{1}{\text{Cos.}^2\theta}\right\};$$

and, the second term of this expression being inconsiderable in comparison of the first, we get, for an approximate value, $r = \alpha(1+\alpha)\,\text{Tan.}\,\theta$; and again, if we substitute r for $\alpha(1+\alpha)\,\text{Tan.}\,\theta$ we shall obtain,

$$n = \frac{i - \frac{1}{2}\alpha}{\alpha(1+\alpha)^2},$$

$$r = \alpha(1+\alpha) \cdot \left\{\text{Tan.}\,\theta - nr \times \frac{d \cdot \text{Tan.}\,\theta}{d\theta}\right\}.$$

But this value of r is no other than the two first terms of the developement of $\alpha(1+\alpha)\,\text{Tan.}\,(\theta - nr)$; and hence,

$$r = \alpha(1+\alpha)\,\text{Tan.}\,(\theta - nr),$$

an expression which must be considered as an approximation of the same order with the formula of Laplace, and it must be restricted within the same limits. It is to be observed, however, that the two forms of expression will not be entirely equivalent unless the same values of α and i be, in every case, substituted in both; which implies that n will vary a little according to the pressure and temperature of the air.

The formula for the refractions near the zenith is common

to every constitution of the atmosphere. In proceeding farther, our reasoning must comprehend all the varieties of temperature and density that actually take place in nature from the surface of the earth to the utmost height at which the air possesses power to refract the rays of light. Even if we should succeed in this, it would be chimerical to expect that a formula can be found that would apply to single observations without great occasional inequalities. This is not to be ascribed to any fault of the theory; it arises from the nature of the observations themselves. If we examine a set of observed refractions, it will be easy to discover instances in which the true refraction has diminished when, according to the instruments employed, it ought to have increased; and, the contrary. The refractions are therefore affected by circumstances of which the observer has no intimation, and which cannot enter into any theory. The real causes of such anomalies is undoubtedly the irregular changes that take place in the remote parts of the atmosphere, which are not indicated by the barometer or the thermometer. We must conceive that the atmosphere is perpetually oscillating about a mean state, which it ought to be the aim of theory to discover. The test of success in the research must be looked for, not in the perfect agreement of the theory with every single instance, but in the disappearance of the unavoidable errors in a sufficient number of observations made at different times.

7. There is no ground in experience for attributing to the gradation of heat in the atmosphere any other law than that of an equable decrease as the altitude increases. This law prevails very nearly at least to the greatest heights to which

we have been able to ascend. The mean elevation for one degree of depression of the centigrade thermometer is very nearly 90 English fathoms; and in the great height ascended by GAY LUSSAC, rather more than $4\frac{1}{4}$ miles, the same quantity comes out equal to 95 fathoms. To this great extent the law of a uniform decrease of temperature holds good, without much deviation from the truth. It therefore seems to be the assumption most likely to guide us aright in approximating to the true constitution of the atmosphere.

The law we have mentioned is expressed by this equation, viz.

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - \frac{s}{m+1};$$

$m+1$ being a constant quantity which, in the case of nature, will be determined by equation (B). Now if we substitute this value of $\frac{1+\beta\tau}{1+\beta\tau'}$ in the formula (A), and then equate the two values of P, we shall get,

$$\left(1-\frac{s}{m+1}\right)\cdot(1-\omega) = \int -ds\,(1-\omega);$$

and, hence,

$$\frac{d\omega}{1-\omega} = \frac{ds}{m+1} \times \frac{m}{1-\frac{s}{m+1}};$$

consequently,

$$1-\omega = \left(1-\frac{s}{m+1}\right)^{m}.$$

Thus we obtain,

$$P = \left(1-\frac{s}{m+1}\right)^{m+1}$$

$$1-\omega = \left(1-\frac{s}{m+1}\right)^{m}$$

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1-\frac{s}{m+1}.$$

In these equations, the hypothesis of CASSINI corresponds to $m=0$; that of a density decreasing uniformly as the altitude

increases, to $m = 1$; and, when m is infinitely great, the same equations become,

$$P = c,^{-s}$$

$$1 - \omega = c,^{-s}$$

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1,$$

c being the base of the hyperbolic logarithms; and they now belong to an atmosphere in which the density is proportional to the pressure, and the heat is the same in every part. These three suppositions, with some modifications of them, are the foundations of all the theories that have been advanced with regard to the variations of density in the atmosphere. They are the simplest cases that come under the foregoing formulæ, and likewise those that are suggested by the most obvious physical hypotheses. But in reality these considerations afford no good ground of preference; since, whatever value we give to m, the general laws relating to the heat and pressure of the air, are equally well represented. The refractions near the zenith will likewise be the same, whatever number m stands for. We may therefore adopt that value of m which will give the true refractions near the horizon; or that one, which will satisfy equation (B), in which case the gradation of heat will coincide with that actually observed at the surface of the earth. More especially if, by the same value of m, we can conciliate both the above-mentioned conditions, we may conclude that the solution of the problem must agree well with observation. But, in order to continue this research, it is necessary to find a method that will enable us to compute the refractions for any proposed value of m.

If we make $z = \frac{s}{m+1}$, then

$$s = m + 1 \cdot z,$$

$$1 - \omega = (1 - z),^{m}$$

$$\omega = 1 - (1 - z),^{m}$$

$$2is - 2\alpha\omega = 2i(m + 1)z - 2\alpha\left\{1 - (1 - z)^{m}\right\}:$$

and, again,

$$\lambda = \frac{\alpha}{i},$$

$$\mathrm{a} = m + 1 - \lambda$$

$$\psi = 1 - (1 - z)^{m-1}$$

$$2is - 2\alpha\omega = 2i\mathrm{a}z - 2i\lambda(1 - z)\psi$$

$$d\omega = m\,dz\,(1 - z)^{m-1}.$$

The expression of the refraction (equation C) will therefore become,

$$r = \alpha(1 + \alpha)\,\mathrm{Sin.}\,\theta \times \int \frac{m\,dz\,(1 - z)^{m-1}}{\sqrt{\mathrm{Cos.}^2\theta + 2i\mathrm{a}z - 2i\lambda(1 - z)\psi}}:$$

and by expanding the radical quantity,

$$\Delta = \sqrt{\mathrm{Cos.}^2\theta + 2i\mathrm{a}z}$$

$$\begin{aligned} r = \alpha(1 + \alpha)\,\mathrm{Sin.}\,\theta \times \Bigg\{ & \int \frac{m\,dz\,(1 - z)^{m-1}}{\Delta} \\ & + i\lambda \int \frac{m\,dz\,(1 - z)^{m}\psi}{\Delta^3} \\ & + \frac{1 \cdot 3}{1 \cdot 2} \cdot i^2\lambda^2 \cdot \int \frac{m\,dz\,(1 - z)^{m+1}\psi^2}{\Delta^5} \\ & + \&c. \end{aligned}$$

And, in this expression, it is not necessary to integrate generally, but merely to find the definite integrals between the limits $z = o$ and $z = 1$.

Now by taking the fluxions of the quantity $\frac{(1-z)^{m+n-1}\psi^n}{\Delta^{2n-1}}$, we have

$$\frac{(1 - z)^{m+n-1}\psi^n}{\Delta^{2n-1}} = -(2n - 1) \cdot i\mathrm{a} \cdot \int \frac{dz\,(1 - z)^{m+n-1}\psi^n}{\Delta^{2n+1}}$$

$$+\int\frac{dz}{\Delta^{2n-1}}\cdot\frac{d\cdot(1-z)^{m+n-1}\psi^{n}}{dz};$$

and because the function without the sign of integration is evanescent at both the limits $z=0$, $z=1$, we shall get, with regard to the definite integrals,

$$\int\frac{dz(1-z)^{m+n-1}\psi^{n}}{\Delta^{2n+1}}=-\frac{1}{2n-1}\cdot\frac{1}{i\,\mathrm{a}}\cdot\int\frac{dz}{\Delta^{2n-1}}\cdot\frac{d\cdot(1-z)^{m+n-1}\psi^{n}}{dz}.$$

By operating in like manner with the quantities $\frac{1}{\Delta^{2n-3}}\cdot\frac{d\cdot(1-z)^{m+n-1}\psi^{n}}{dz}$ and $\frac{1}{\Delta^{2n-5}}\cdot\frac{dd\cdot(1-z)^{m+n-1}\psi^{n}}{dz^{2}}$, we shall obtain,

$$\int\frac{dz}{\Delta^{2n-1}}\cdot\frac{d\cdot(1-z)^{m+n-1}\psi^{n}}{dz}=\frac{1}{2n-3}\cdot\frac{1}{i\,\mathrm{a}}\cdot\int\frac{dz}{\Delta^{2n-3}}\cdot\frac{dd\cdot(1-z)^{m+n-1}\psi^{n}}{dz^{2}}$$

$$\int\frac{dz}{\Delta^{2n-3}}\cdot\frac{dd\cdot(1-z)^{m+n-1}\psi^{n}}{dz^{2}}=\frac{1}{2n-5}\cdot\frac{1}{i\,\mathrm{a}}\cdot\int\frac{dz}{\Delta^{2n-5}}\cdot\frac{d^{3}(1-z)^{m+n-1}\psi^{n}}{dz^{3}}.$$

And if we continue the like operations till we come to the quantity, $\frac{1}{\Delta}\cdot\frac{d^{n}\cdot(1-z)^{m+n-1}\psi^{n}}{dz^{n}}$, which is no longer divisible by ψ; and then combine all the results, we shall get,

$$\int\frac{dz(1-z)^{m+n-1}\psi^{n}}{\Delta^{2n+1}}=\frac{1}{1\cdot3\cdot5\ldots2n-1}\cdot\frac{1}{i^{n}\mathrm{a}^{n}}\cdot\int\frac{dz}{\Delta}\cdot\frac{d^{n}.(1-z)^{m+n-1}\psi^{n}}{dz^{n}}.$$

By the application of this formula all the integrals in the value of r will be reduced to others in which the exponent of Δ is unit; viz.

$$\begin{aligned}r=\alpha(1+\alpha)\,\mathrm{Sin.}\,\theta\times\Bigg\{&\int\frac{m\,dz(1-z)^{m-1}}{\Delta}\\&+\lambda.m.\int\frac{dz}{\Delta}\cdot\frac{d.(1-z)^{m}\psi}{\mathrm{a}.dz}\\&+\frac{\lambda^{2}}{1.2}\cdot m\cdot\int\frac{dz}{\Delta}\cdot\frac{dd.(1-z)^{m+1}\psi^{2}}{\mathrm{a}^{2}.dz^{2}}\\&+\frac{\lambda^{3}}{1.2.3}\cdot m\cdot\int\frac{dz}{\Delta}\cdot\frac{d.^{3}(1-z)^{m+2}\psi^{3}}{\mathrm{a}^{3}.dz^{3}}\\&+\&\mathrm{c.}\end{aligned}$$

In the extreme case when m is infinitely great, we have

and,
$$1 - \omega = c^{-s};$$

$$r = \alpha\,(1 + \alpha)\ \text{Sin.}\ \theta \times \int \frac{d s c^{-s}}{\sqrt{\text{Cos.}^2\theta + 2 i s - 2 i \lambda\,(1 - c^{-s})}}:$$

and if we expand this expression and apply the like reasoning as before, we shall obtain

$$\Delta = \sqrt{\text{Cos.}^2\theta + 2 i s}$$

$$\begin{aligned} r = \alpha\,(1 + \alpha)\ \text{Sin.}\ \theta \times \Big\{ & \int \frac{d s c^{-s}}{\Delta} \\ & + \lambda . \int \frac{d s}{\Delta} . \frac{d . c^{-s}(1 - c^{-s})}{d s} \\ & + \frac{\lambda^2}{1 . 2} . \int \frac{d s}{\Delta} . \frac{d d . c^{-s}(1 - c^{-s})^2}{d s^2} \\ & + \frac{\lambda^3}{1 . 2 . 3} . \int \frac{d s}{\Delta} . \frac{d^3 . c^{-s}(1 - c^{-s})^3}{d s^3} \\ & + \&c. \end{aligned}$$

an expression which has already been given by KRAMP and LAPLACE, and is no other than the limit of the foregoing formula when m is infinitely great.

The calculation of the refractions is now reduced to such integrals as $\int \frac{d z\,(1 - z)^{p-1}}{\Delta}$, p being any number; and the valuing of these must next engage our attention.

8. In the first place, when $\theta = 90°$, as in the case of the refractions at the horizon, then $\text{Cos.}^2\,\theta = 0$, and $\Delta = \sqrt{2 i a z}$: now, put $z = t^2$, and

$$\int \frac{d z\,(1 - z)^{p-1}}{\Delta} = \frac{2}{\sqrt{2 i a}} . \int d t\,(1 - t^2)^{p-1},$$

the integral being taken between the limits $t = 0$ and $t = 1$.

When p is a whole number,

$$\int d t\,(1 - t^2)^{p-1} = \frac{2 . 4 . 6 \ldots 2\,(p - 1)}{3 . 5 . 7 \ldots 2 p - 1};$$

which will apply conveniently in all cases unless when p is a great number.

When p is great, assume

$$1 - t^2 = c^{-\frac{x^2}{p-1}}:$$

then

$$t^2 = \frac{x^2}{p-1} - \frac{1}{2} \cdot \frac{x^4}{(p-1)^2} + \frac{1}{6} \cdot \frac{x^6}{(p-)^3} - \frac{1}{24} \cdot \frac{x^8}{(p-1)^4} + \&c.:$$

and by extracting the square root,

$$t = \frac{1}{\sqrt{p-1}} \times \left\{ x - \frac{1}{4} \cdot \frac{x^3}{p-1} + \frac{5}{96} \cdot \frac{x^5}{(p-1)^2} \right.$$
$$\left. - \frac{1}{128} \cdot \frac{x^7}{(p-1)^3} + \frac{79}{92160} \cdot \frac{x^9}{(p-1)^4} - \&c. \right\}.$$

Hence,

$$\int dt(1-t^2)^{p-1} = \int \frac{dx\, c^{-x^2}}{\sqrt{p-1}} \cdot \left\{ 1 - \frac{3}{4} \cdot \frac{x^2}{p-1} + \frac{25}{96} \cdot \frac{x^4}{(p-1)^2} - \&c. \right\}:$$

now, the limits of the integrals being $t = o$, $t = 1$, and $x = o$, $x = \infty$; we get

$$\int dt(1-t^2)^{p-1} = \frac{1}{2} \cdot \frac{\sqrt{\pi}}{\sqrt{p-1}} \times \left\{ 1 - \frac{3}{8} \cdot \frac{1}{p-1} \right.$$
$$\left. + \frac{25}{128} \cdot \frac{1}{(p-1)^2} - \frac{105}{1024} \cdot \frac{1}{(p-1)^3} + \frac{1659}{32768} \cdot \frac{1}{(p-1)^4} - \&c. \right\}.$$

By employing proper reductions, any proposed case may be brought to another in which this series will converge swiftly.

In the next place, when $\text{Cos.}^2\,\theta$ is not evanescent, put

$$z = u - e^2(u - u^2);$$

then,

$$(1-z)^{p-1} = (1-u)^{p-1} . (1 + e^2 u)^{p-1}$$
$$\Delta = \sqrt{\text{Cos.}^2\,\theta + 2\,i\,\text{a}\,(1-e^2)\,u + 2\,i\,\text{a}\,e^2 u^2}:$$

in order to determine e, assume,

$$\Delta = \text{Cos.}\,\theta + e\,u\sqrt{2\,i\,\text{a}};$$

then,

$$\frac{\sqrt{2\,i\,\text{a}}}{\text{Cos.}\,\theta} = \frac{2e}{1-e^2}$$
$$\frac{dz}{\Delta} = \frac{2e}{\sqrt{2\,i\,\text{a}}} \times du.$$

Hence,

$$p\int\frac{dz(1-z)^{p-1}}{\Delta}=\frac{2e}{\sqrt{2ia}}\cdot p\,du\,.\,(1-u)^{p-1}.\,(1+e^2u)^{p-1};$$

consequently,

$$\begin{aligned}p\,.\int\frac{dz(1-z)^{p-1}}{\Delta}=\frac{2e}{\sqrt{2ia}}\times\Big\{&p\textstyle\int du\,(1-u)^{p-1}\\&+e^2\,.\,p\,.\,p-1\,.\textstyle\int du\,.\,u(1-u)^{p-1}\\&+e^4\,.\,p\,.\,p-1\,.\,\frac{p-2}{2}\,.\textstyle\int du\,.\,u^2(1-u)^{p-1}\\&+\text{\&c.}\end{aligned}$$

and, by integrating between the limits $u=0$, $u=1$,

$$p\int\frac{dz(1-z)^{p-1}}{\Delta}=\frac{2}{\sqrt{2ia}}\,.\,\Big\{e+\frac{p-1}{p+1}e^3+\frac{p-1\,.\,p-2}{p+1\,.\,p+2}\,.\,e^5+\text{\&c.}\Big\}.$$

This series will stop when p is a whole number; and e being always less than 1, it will converge fast unless when p is a very great number.

9. The horizontal refraction has not been determined by astronomers with much exactness. The quantity most generally adopted is 33′ 46″.3, which is that of the French tables, and is very little different from the determination of Bradley: it supposes the mean temperature of 50° of Fahrenheit and the barometrical pressure equal to 29.92 English inches. At the same temperature, and with the mean pressure 30 inches, it is equal to

2031″.5.

If we would compare with this the horizontal refraction in the hypothesis of Cassini, we have only to substitute in the formula found in No. 6, the values of α and i given in No. 4: the result will come out equal to

1218″.6.

The case, when the density decreases in the same proportion that the altitude increases, corresponds to $m=1$ in the

formula of No. 7; and ψ being $= o$, we get,

$$r = \alpha\,(1 + \alpha)\ \text{Sin.}\,\theta \times \int \frac{dz}{\Delta};$$

and, at the horizon,

$$r = \alpha(1 + \alpha)\,.\int \frac{dz}{\sqrt{2iaz}} = \frac{2\alpha\,(1 + \alpha)}{\sqrt{2ia}}.$$

Now, a being equal to $m + 1 - \lambda$, we have in this case $a = 2 - \lambda$; wherefore,

$$r = \frac{2\alpha\,(1 + \alpha)}{\sqrt{2i(2 - \lambda)}} = \frac{\alpha\,(1 + \alpha)}{\sqrt{i\left(1 - \frac{\lambda}{4}\right)}} = 1671''.$$

In both these hypotheses, although the refractions near the zenith agree with nature, yet, at the horizon, they fall greatly short of observation.

At the other extreme, when m is infinitely great, the term which is multiplied by λ^n in the expression of the refraction given in No. 7, is thus expressed, viz.

$$\frac{\lambda^n}{1.2.3\ldots n} \times \int \frac{ds}{\Delta}\cdot\ \frac{d^n.\,c^{-s}\left(1 - c^{-s}\right)^n}{ds^n}:$$

but, at the horizon, $\Delta = \sqrt{2is}$; therefore,

$$\frac{\lambda^n}{1.2.3\ldots n} \times \frac{1}{\sqrt{2i}} \times \int \frac{ds}{\sqrt{s}}\cdot\ \frac{d^n.\,c^{-s}\left(1 - c^{-s}\right)^n}{ds^n}:$$

and, by expanding and performing the operations indicated, the same term will become

$$\frac{\lambda^n}{1.2.3\ldots n} \times \frac{1}{\sqrt{2i}} \times \int \frac{ds}{\sqrt{s}}\cdot\ \left\{\pm c^{-s} \mp n\,.\,2^n.\,c^{-2s} \pm n.\frac{n-1}{2}\cdot 3^n c^{-3s} \mp \&\text{c.}\ \right\},$$

the upper or lower sign taking place according as n is even or odd. If now we put $s = t^2$, and then integrate between the limits $t = o$, $t = \infty$, we shall get,

$$\frac{\lambda^n}{1.2.3\ldots n} \times \frac{\sqrt{\pi}}{\sqrt{2i}} \times \left\{\pm 1 \mp n\,.\,\frac{2^n}{\sqrt{2}} \pm n.\,\frac{n-1}{2}\cdot\frac{3^n}{\sqrt{3}} \mp \&\text{c.}\ \right\}.$$

Hence if, in the case of the horizontal refraction, we assume

$$r = \frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}} \times \left\{1 + A^{(1)}\lambda + A^{(2)}\lambda^2 \ldots . A^{(n)}\lambda^n . \&c. \right\},$$

we shall have

$$A^{(n)} = \frac{1}{1.2.3..n} \times \left\{\pm 1 \mp n . \frac{2^n}{\sqrt{2}} \pm n . \frac{n-1}{2} . \frac{3^n}{\sqrt{3}} \mp \&c. \right\}.$$

By means of this formula Kramp has found,

$$A^{(1)} = 0.414214,$$
$$A^{(2)} = 0.269649,$$
$$A^{(3)} = 0.200865,$$
$$A^{(4)} = 0.160253,$$
$$A^{(5)} = 0.132935,$$
&c.

And with these values the horizontal refraction, in an atmosphere of uniform temperature, will come out equal to

$$2254''.5.$$

In this case, therefore, the refractions, at the horizon greatly exceed the truth, although at the zenith they agree with observation.

It is therefore certain that if we augment m, by successively putting $m = 2$, $m = 3$, &c., we shall at length find an atmosphere that will agree with nature both at the zenith and the horizon. But if we reflect that there must be an intimate connection between the quantity of the refractions and the gradation of heat in the atmosphere, we shall probably be spared some repetitions of the same operations, by determining m so as to satisfy Equation (B). Now we have

$$1 - \omega = \left(1 - \frac{s}{m+1}\right)^m;$$

consequently,

$$\frac{d\omega}{ds}\left(\text{when } s = 0\right) = \frac{m}{m+1} = \frac{4}{5};$$

and hence $m = 4$.

But in the formula of No. 7, when $m = 4$, $a = 5 - \lambda$, $\psi = 1 - (1 - z)^3$; and if we perform the operations indicated, and for the sake of brevity put

$$Q^{(1)} = 4 \int \frac{dz\,(1-z)^3}{\Delta}$$
$$Q^{(2)} = 7 \int \frac{dz\,(1-z)^6}{\Delta}$$
$$Q^{(3)} = 10 \int \frac{dz\,(1-z)^9}{\Delta}$$
$$Q^{(4)} = 13 \int \frac{dz\,(1-z)^{12}}{\Delta}$$

we shall get,

$$r = \alpha\,(1 + \alpha)\,\text{Sin.}\,\theta \times \Big\{ Q^{(1)} + \lambda \cdot \frac{4}{5-\lambda} \cdot (-Q^{(1)} + Q^{(2)})$$
$$+ \frac{\lambda^2}{1.2} \cdot \frac{4}{5-\lambda} \cdot \frac{5\,Q^{(1)} - 16\,Q^{(2)} + 11\,Q^{(3)}}{5-\lambda}$$
$$+ \frac{\lambda^3}{1.2.3} \cdot \frac{4}{5-\lambda} \cdot \frac{-30\,Q^{(1)} + 216\,Q^{(2)} - 396\,Q^{(3)} + 210\,Q^{(4)}}{(5-\lambda)^2}$$
$$+ \&c.$$

This is the general value of the refraction when $m = 4$: but, at the horizon, we get

$$Q^{(1)} = \frac{2}{\sqrt{2i(5-\lambda)}} \times 4 \int dt\,(1-t^2)^3 = \frac{2}{\sqrt{2i(5-\lambda)}} \times \frac{64}{35}$$
$$Q^{(2)} = \frac{2}{\sqrt{2i(5-\lambda)}} \times 7 \int dt\,(1-t^2)^6 = \frac{2}{\sqrt{2i(5-\lambda)}} \times \frac{1024}{429}$$
$$Q^{(3)} = \frac{2}{\sqrt{2i(5-\lambda)}} \times 10 \int dt\,(1-t^2)^9 = \frac{2}{\sqrt{2i(5-\lambda)}} \times \frac{131072}{46189}$$
$$Q^{(4)} = \frac{2}{\sqrt{2i(5-\lambda)}} \times 13 \int dt\,(1-t^2)^{12} = \frac{2}{\sqrt{2i(5-\lambda)}} \times \frac{4194304}{1300075}:$$

and, with these values, the series for the horizontal refraction will become

$$r = \frac{2\alpha\,(1+\alpha)}{\sqrt{2i(5-\lambda)}} \times \Big\{ 1.82857 + \lambda \times 0.46717$$
$$+ \lambda^2 \times 0.18959$$
$$+ \lambda^3 \times 0.08836$$
$$+ \&c.$$

and by completing the calculation, we shall get,

$$r = 2041''.3.$$

This result is very near 2031″ 5, the horizontal refraction usually adopted; the difference 9″.8 being much less than the uncertainty in the determination of this quantity. But it will be more satisfactory to compare the refractions in this hypothesis at all altitudes with those admitted by astronomers. In order to find a formula for this purpose we have only to substitute for $Q^{(1)}$, $Q^{(2)}$, $Q^{(3)}$, the series investigated in No. 8; and we may leave out the term multiplied by λ^3, since the amount of it is less than 1″ even at the horizon. Thus we get,

$$\frac{\sqrt{2i(5-\lambda)}}{\text{Cos. }\theta} = \frac{2e}{1-e^2};$$

$$\begin{aligned} r = \frac{2\alpha(1+\alpha)\text{Sin. }\theta}{\sqrt{2i(5-\lambda)}} \times \Big\{ & e + \frac{3}{5}e^5 + \frac{1}{5}e^5 + \frac{e^7}{35} \\ & + \frac{4\lambda}{5-\lambda} \cdot \left(\frac{3}{20}e^3 + \frac{13}{60}e^5 + \frac{19}{210}e^7 + \frac{e^9}{22} + \frac{e^{11}}{132}\right) \\ & + \frac{2 \,.\, \lambda^2}{(5-\lambda)^2} \cdot \left(\frac{1}{3}e^5 + \frac{193}{273}e^7 + \frac{94}{243}e^9 + \frac{146}{429}e^{11}\right)\Big\} \end{aligned}$$

And, by substituting the numerical values, we shall find,

$$\text{Tan. }\varphi = 19.0462371 + \text{Sec. }\theta - 20.$$

$$\begin{array}{rrll} & '' & & \text{Log. of Coeff.} \\ r = & 1048.95 \times & \text{Tan. } \tfrac{1}{2}\varphi \text{ Sin. }\theta & \ldots\ 3.0207558 \\ + & 658.21 \times & \text{Tan.}^3 \tfrac{1}{2}\varphi \text{ Sin. }\theta & \ldots\ 2.8183661 \\ + & 252.92 \times & \text{Tan.}^5 \tfrac{1}{2}\varphi \text{ Sin. }\theta & \ldots\ 2.4029800 \\ + & 59.64 \times & \text{Tan.}^7 \tfrac{1}{2}\varphi \text{ Sin. }\theta & \ldots\ 1.7755092 \\ + & 11.61 \times & \text{Tan.}^9 \tfrac{1}{2}\varphi \text{ Sin. }\theta & \ldots\ 1.0648048 \\ + & 2.95 \times & \text{Tan.}^{11} \tfrac{1}{2}\varphi \text{ Sin. }\theta & \ldots\ 0.4706968 \end{array}$$

But it is to be observed, that the logarithm of $\frac{30}{29.921}$ has been subtracted from the logarithm of every coefficient, in order to bring the formula to the same barometrical pressure with

the Table of Refractions published in the *Connaissance des Tems,* with which it is proposed to compare it. The comparison is contained in the following Table:

Zen. Dist.	Formula.	Con. Des. T.	Diff.
°	′ ″	′ ″	″
45	0 58.2	58.2	0
60	1 40.6	1 40.6	0
70	2 38.8	2 38.8	0
80	5 19.3	5 19.8	0.5
85	9 51.7	9 54.3	2.6
86	11 44.2	11 48.3	4.1
87	14 21.5	14 28.1	6.6
88	18 11.9	18 22.2	10.3
89	24 8.6	24 21.2	12.6
90	33 54.3	33 46.3	— 8.0

The formula agrees exactly with the table till 80° of zenith distance, when the difference is 0″.5. But if we turn to the *Tables Astronomiques,* published in 1806, by the French Board of Longitude, we shall find that there is a small correction to be subtracted from the mean refractions; and when this is taken into account, the perfect agreement between the formula and the table will be restored. In like manner there are subtractive corrections to be applied at all other zenith distances; and these increase very swiftly in approaching the horizon. To explain the reason of this, it must be observed that the French table was originally constructed for 32° of Fahrenheit, and was reduced to the mean temperature of 50°, on the supposition that the refractions vary in the same proportion with the density of the air; by which procedure the change in their quantity that arises from the variations of the

elementary quantities in the algebraic formula is neglected. Had the computation been rigorously made, as BESSEL has since done in the table he published in 1818, the mean refractions of the French table would have been less, instead of being greater, than the results of the foregoing formula. But it was the opinion of the eminent astronomers under whose direction the table was published, that the refractions near the horizon are too uncertain to require attention to minute accuracy.

It appears therefore, as far as we can form an exact judgment, that the formula approaches very near the true mean refractions. It will afterwards be shown that the hypothesis from which it has been deduced, likewise represents, with considerable accuracy, the pressures and densities actually observed in the atmosphere at different heights. But in one respect there is a deviation from nature. According to the supposition $m = 4$, the total height of the atmosphere is equal to $5 \times l$, or about 25 miles, which, in all probability, is hardly equal to half the real height. It therefore becomes necessary to inquire what influence this circumstance will have on the quantity of the refractions.

10. Continuing to represent the density, or $1 - \omega$, by $(1 - z)^m$, we may assume

$$P = (1 - f) \cdot (1 - z)^{m+1} + f(1 - z)^{2m},$$

f being an arbitrary quantity. Then, from the formulæ (A), we get $s = \int \frac{-dP}{1 - \omega}$, and $\frac{1 + \beta\tau}{1 + \beta\tau'} = \frac{P}{1 - \omega}$; and hence

$$s = (m + 1) \cdot (1 - f) \cdot z + 2f \cdot \left\{1 - (1 - z)^m\right\},$$

$$\frac{1 + \beta\tau}{1 + \beta\tau'} = (1 - z) \cdot \left\{1 - f + f(1 - z)^{m-1}\right\};$$

and it is to be observed, that this last quantity is always

evanescent at the top of the atmosphere, however great m is supposed to be. We likewise get

$$\frac{d\omega}{ds}\,(\text{making } s = 0) = \frac{m}{m+1+(m-1)f} = \frac{4}{5};$$

wherefore

$$f = \frac{1}{4}\cdot\frac{m-4}{m-1}.$$

In this formula, $f = 0$, when $m = 4$, which is the case already considered; and $f = \frac{1}{4}$, when m is infinitely great. Between these two extreme cases, there are contained an infinite number of atmospheres gradually extending higher above the earth's surface, till the total height from being about 25 miles becomes unlimited. In all these different atmospheres $\frac{d\omega}{ds}$ has the same value when $s = 0$; and therefore they all agree with one another, and with nature, in having the same gradation of heat at the earth's surface. But the rate at which the heat decreases is different in every one; being equable only when $m = 4$, and in all the rest becoming slower as the height increases. As all this is easily made out from the foregoing equations, it will not be necessary to enter into any detail on the subject.

When m is less than 4, f becomes negative: but these cases are excluded, since they belong to atmospheres still less elevated than when $m = 4$. They are excluded too for another reason: for, although the rate of the decrease of heat at the earth's surface agrees with nature, yet it increases in ascending, which is contrary to experience.

It remains to determine the refractions in the different atmospheres included in the formula. As we already know the horizontal refraction in one extreme case, it will be sufficient to seek its amount in the other extreme case. Now, if we put

$u = mz$, then, supposing m infinitely great, we get

$$1 - \omega = (1 - z)^m = c^{-u},$$

$$s = (1 - f)\,u + 2f\,(1 - c^{-u}):$$

consequently,

$$2is - 2\alpha\omega = 2i(1 - f)\,.\,u + 2i\,(2f - \lambda)\,.\,(1 - c^{-u}):$$

and, if we make $\mathrm{a} = (1 - f)\,k = \frac{2f - \lambda}{1 - f}$; then $2is - 2\alpha\omega = 2i\mathrm{a}\,.\,u + 2i\mathrm{a}\,.\,k\,(1 - c^{-u})$.

Hence,

$$r = \alpha\,(1 + \alpha)\,\mathrm{Sin.}\,\theta \times \int \frac{du\,c^{-u}}{\sqrt{\mathrm{Cos.}^2\,\theta + 2i\mathrm{a}\,.\,u + 2i\mathrm{a}\,.\,k\,(1 - c^{-u})}}.$$

Applying to this expression the method already employed in No. 9, we shall get, in the case of the horizontal refraction,

$$r = \frac{\alpha(1 + \alpha)\sqrt{\pi}}{\sqrt{2i(1 - f)}} \times \left\{1 - \mathrm{A}^{(1)}\,.\,k + \mathrm{A}^{(2)}\,.\,k^2 - \mathrm{A}^{(3)}k^3 + \mathrm{A}^{(4)}\,.\,k^4 - \mathrm{A}^{(5)}k^5\right\}.$$

Now, $f = \frac{1}{4}$; $\lambda = 0.21909$; $k = 0.37455$: with these numbers r comes out equal to 2059″.7 when all the terms of the series that are set down, are taken in except the last; and to 2057″.4, when all the terms are taken in: the more correct value of r is therefore 2058″.5, which is just 17″.2 more than in the other extreme case of $m = 4$. Thus the refractions undergo hardly any change in all the atmospheres comprehended in the formula; although their height increases from about 25 miles to be infinitely great; and although the rate of the decrease of heat, which has always the same initial value, varies differently in each.

Reflecting on what has just been proved, it is extremely probable that, for every value of m between the two extreme cases, the densities and pressures will be found, at least to a great height, very nearly the same as in the real atmosphere. We can hardly account, on any other supposition, for the

near coincidence of the refractions in so many different cases with the observed quantities. In order to examine this point, we may take the case of GAY LUSSAC's ascent; the data obtained by observation, as they are given by RAMOND,* being as follows, viz.

$$\text{Log. } P = -\ 1.6361109$$
$$\tau' = 30°.8$$
$$\tau = -\ 9.5.$$

With these numbers, by means of the formula $P = \frac{1+\beta\tau}{1+\beta\tau'} \times (1-\omega)$, we get,

$$1-\omega = 0.5004,$$

which may be reckoned the density by observation; and we must now compare it with the result of the theory.

Now when $m = 4$, $f = 0$; and we have these equations, viz.

$$P = \left(1 - \frac{s}{5}\right)^5$$
$$1-\omega = \left(1 - \frac{s}{5}\right)^4;$$

consequently,

$$1-\omega = P^{\frac{4}{5}}:$$

and, by substituting the foregoing value of P, we find,

$$1-\omega = 0.5115,$$

which is greater than the value deduced from observation by about $\frac{1}{50}$ of the whole.

Again, we have generally,

$$1-\omega = (1-z)^m$$
$$P = (1-f)(1-z)^{m+1} + 2f(1-z)^{2m}:$$

wherefore,

$$P = (1-f)(1-\omega)^{1+\frac{1}{m}} + 2f(1-\omega)^2;$$

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* Memoires sur la Formule Barometrique, 1811. Examples at the end.

and, when m is infinite, $f = \frac{1}{4}$,

$$P = \tfrac{3}{4}(1-\omega) + \tfrac{1}{4}(1-\omega)^2.$$

By solving this equation with the given value of P, we get,

$$1 - \omega = 0.4951,$$

which is less than the true quantity by about $\frac{1}{100}$ of the whole.

It is remarkable that the two results lie on opposite sides of the true quantity: from which it follows, that a value of m greater than 4 may be found, that will accord exactly with the observation of Gay Lussac. No confidence, however, could be placed in a calculation founded on a single instance, where an enormous difference in the results would be produced by a small error in the quantities determined by experiment. But at any rate, what has just been remarked, agrees very well with all the arguments that have been advanced to prove the finite extent of the atmosphere surrounding the earth.

For farther illustration, some other observed heights have been selected from the same work of Ramond, the calculations being made in the same manner. The results are contained in the following table.

Places.	By Observation.				By Theory.		Heights.
	Logarithms P.	τ'	τ	Density.	Density.		Fathoms.
					$m = 4$	$m = \infty$	
Puy de Dome, in Auvergne,	—1.94529	18°.6	11°.7	0.9035	0.9041	0.9035	583
Mont Perdu, in the Pyrenees,	—1.88944	20	7.5	0.8106	0.8157	0.8132	1185
Pic du Midi, High Pyrenees.	—1.86738	25.4	10.4	0.7768	0.7833	0.7798	1429
Etna - - - -	—1.82811	23.1	4.4	0.7196	0.7286	0.7232	1825
Chimborazo, in the Andes,	—1.69582	25.3	— 1.6	0.5468	0.5710	0.5580	3215
Gay Lussac's ascent,	—1.63611	30.8	— 9.5	0.5004	0.5115	0.4951	3816

It appears, therefore, that in all the atmospheres comprehended in the assumed formula, the density corresponding to a given pressure and temperature coincides very nearly with what is actually found by experiment. But although this be admitted, it may still be questioned, whether the height at which any proposed pressure takes place will agree equally well with observation. Now, in the real atmosphere, the height belonging to any pressure is usually deduced from the formula for barometrical measurements; and it will be sufficient to show, that the same formula is true in all the atmospheres we are considering.

In the first place, when $m = 4$, we have

$$P = \left(1 - \frac{s}{5}\right)^5$$

$$\frac{1 + \beta\tau}{1 + \beta\tau'} = 1 - \frac{s}{5}.$$

From the first of these equations we get

$$\text{Log.}\,\frac{1}{P} = 5 \log.\,\frac{1}{1 - \frac{s}{5}} = s\left(1 + \tfrac{1}{2} \cdot \frac{s}{5}\right),$$

neglecting the cube and the higher powers of s: and hence,

$$s = \left(1 - \tfrac{1}{2} \cdot \frac{s}{5}\right) \log.\,\frac{1}{P}.$$

But, from the other equation, we get

$$1 + \frac{1 + \beta\tau}{1 + \beta\tau'} = 2 - \frac{s}{5};$$

and hence,

$$\frac{1 + \beta \,.\, \frac{\tau + \tau'}{2}}{1 + \beta\tau'} = 1 - \tfrac{1}{2} \cdot \frac{s}{5}.$$

Now substitute this value in the foregoing equation, and likewise, for s, write the equivalent quantity $\frac{x}{l(1 + \beta\tau')}$; and we shall obtain,

$$x = l \times \left\{1 + \beta \cdot \frac{\tau + \tau'}{2}\right\} \cdot \text{Log.}\,\frac{1}{\text{P}},$$

which is no other than the usual formula for barometrical measurements, x being the height corresponding to the relative pressure P.

Again, when m is infinite, we have already found,

$$1 - \omega = c^{-u},$$

$$\text{P} = (1 - f)c^{-u} + fc^{-2u} = c^{-u} \cdot \left\{1 - f\left(1 - c^{-u}\right)\right\},$$

$$s = (1 - f)u + 2f\left(1 - c^{-u}\right)$$

$$\frac{1 + \beta\tau}{1 + \beta\tau'} = \frac{\text{P}}{1 - \omega} = 1 - f\left(1 - c^{-u}\right).$$

Hence,

$$\text{Log.}\,\frac{1}{\text{P}} = u + \log.\,\frac{1}{1 - f\left(1 - c^{-u}\right)};$$

and, neglecting the cube and higher powers of $1 - c^{-u}$,

$$\text{Log.}\,\frac{1}{\text{P}} = u + f\left(1 - c^{-u}\right) + \frac{f^2}{2} \cdot \left(1 - c^{-u}\right)^2.$$

But, we easily get,

$$\frac{1 + \beta \cdot \frac{\tau + \tau'}{2}}{1 + \beta\tau'} = 1 - \tfrac{1}{2}f\left(1 - c^{-u}\right);$$

wherefore, by multiplying,

$$\text{Log.}\,\frac{1}{\text{P}} \times \frac{1 + \beta \cdot \frac{\tau + \tau'}{2}}{1 + \beta\tau'} = u + f\left(1 - c^{-u}\right) - \frac{f}{2}\,u\left(1 - c^{-u}\right).$$

Now, because

$$fu = f\left(1 - c^{-u}\right) + \frac{f}{2}\left(1 - c^{-u}\right)^2 + \&\text{c.}$$

we get, by substituting and neglecting the same powers of $\left(1 - c^{-u}\right)$ as before,

$$\text{Log.}\,\frac{1}{\text{P}} \times \frac{1 + \beta \cdot \frac{\tau + \tau'}{2}}{1 + \beta\tau'} = u + f\left(1 - c^{-u}\right) - \frac{f}{2}\left(1 - c^{-u}\right)^2.$$

Farther,

$$s = u - fu + 2f\left(1 - c^{-u}\right);$$

but,

$$fu = f(1 - c^{-u}) + \frac{f}{2}(1 - c^{-u})^2;$$

consequently,

$$s = u + f(1 - c^{-u}) - \frac{f}{2}(1 - c^{-u})^2.$$

We have therefore

$$s = \frac{1}{P} \times \frac{1 + \beta \cdot \frac{\tau + \tau'}{2}}{1 + \beta\tau'};$$

and because $s = \frac{x}{l(1 + \beta\tau')}$, we get as before

$$x = l \times \log. \frac{1}{P} \times \left\{1 + \beta \cdot \frac{\tau + \tau'}{2}\right\}.$$

The very exact coincidence in the properties of all the atmospheres comprehended in the assumed formula, with the phenomena actually observed at the surface of the earth, accounts in a satisfactory manner for the near approach of the refractions in every case to the quantities determined by astronomers. It appears that, although the refractions near the zenith are affected in a degree hardly perceptible by the peculiar constitution of the atmosphere, yet, near the horizon, they depend entirely on the same arrangement of the strata of air indicated by terrestrial experiments. The causes of the irregularities observable in these last, likewise disturb the celestial phenomenon in a more remarkable manner. In measuring the height of a column of air, the accidental disturbances to which the atmosphere is continually subject, are in some measure corrected by means of the temperatures observed at both extremities of the column; but, in computing the refractions, the astronomer has no guide but the thermometer placed at his Observatory. In the remote parts of the atmosphere, there may occur innumerable changes deflecting the light of a star from its proper course, of which

he has no intimation, and for which he can make no allowance. In comparison of this great source of error we may reckon as of small account, the inaccuracies that are owing to the neglect of the moisture diffused in the atmosphere, or to our want of an exact knowledge of the law of density in regard to temperature. There can hardly be any other remedy than that of which astronomers so often avail themselves, whenever an ignorance of the real causes obliges them to assimilate the phenomena to the effect of chance; namely, to multiply observations in different circumstances, with the view of making the inequalities of an opposite description compensate one another.

From the foregoing discussion we may draw this conclusion: that an atmosphere constituted like that of the earth, must have an altitude of at least 25 miles, in order that the refractions from the zenith to the horizon be such as they are actually observed to be. But an atmosphere agreeing with nature in the quantity of the refractions may be found, that shall have any proposed altitude greater than the minimum quantity.

We may infer from the duration of the twilight, that the atmosphere of the earth must have an altitude equal to 50 miles, or even more; which corresponds to the supposition of m equal to, or greater than 10. But all these cases are so little different, as to the refractions, from the extreme case when m is infinitely great, that we may suppose them to coincide with it. The most probable supposition with respect to the mean law of density, seems therefore to be contained in these equations, viz.

$$1 - \omega = c^{-u}$$

$$s = (1 - f)u + 2f(1 - c^{-u});$$

f being nearly equal to $\frac{1}{4}$. In this hypothesis it has been shown that the same pressures, densities, and temperatures, would take place at the same altitudes as in the real atmosphere, as far at least as observation enables us to determine. We may therefore presume, that it is not far from that mean state which would prevail, if the regular disposition of the strata of air were not continually deranged by disturbing causes. It remains now to find the refractions in this hypothesis, and to compare them with the quantities observed by astronomers.

12. We have hitherto supposed that $\alpha, i, \lambda = \frac{\alpha}{i}$, are quantities varying with the pressure and temperature of the air; but it will now be necessary to restrict those symbols to the particular values that take place at the mean temperature of 50° of FAHRENHEIT, and the mean pressure of 30 English inches. We shall use the expressions $\alpha\left(1+\frac{\delta\alpha}{\alpha}\right), i\left(1+\frac{\delta i}{i}\right)$, $\lambda + \delta\lambda$, to denote the like quantities as altered by the changes in the atmosphere. What was before signified by

$$2is - 2\alpha\omega, \text{ or } 2i(s - \lambda\omega),$$

will now be thus written, viz.

$$2i\left(1+\frac{\delta i}{i}\right).\left\{s-(\lambda+\delta\lambda)\,\omega\right\};$$

and, by substituting the assumed value of s, and rejecting quantities of the second order with regard to the variations, the same expression will become,

$$\begin{aligned} &2i.\left\{(1-f)u+2f.c^{-u}\right\}\\ &+2i.\frac{\delta i}{i}.\left\{(1-f)u+2fc^{-u}\right\}\\ &-2i.\delta\lambda.(1-c^{-u});\end{aligned}$$

or, more simply,

$$2iy + 2i \cdot \frac{\delta i}{i} y - 2i\delta\lambda(1 - c^{-u}),$$

by putting $y = (1-f)u + 2fc^{-u}$. We shall therefore have this expression of the refraction, viz.

$$r = \left(1 + \frac{\delta\alpha}{\alpha}\right) \cdot \alpha(1+\alpha)\,\text{Sin.}\,\theta \times \int \frac{duc^{-u}}{\sqrt{\text{Cos.}^2\theta + 2iy + 2i \cdot \frac{\delta i}{i} y - 2i\delta\lambda(1-c)^{-u}}}$$

and by expanding

$$r = \left(1 + \frac{\delta\alpha}{\alpha}\right) \cdot \alpha(1+\alpha)\,\text{Sin.}\,\theta \times \int \frac{duc^{-u}}{\sqrt{\text{Cos.}^2\theta + 2iy}}$$

$$- i \cdot \frac{\delta i}{i} \cdot \alpha(1+\alpha)\,\text{Sin.}\,\theta \times \int \frac{duc^{-u}y}{\left\{\text{Cos.}^2\theta + 2iy\right\}^{\frac{3}{2}}}$$

$$+ i\delta\lambda \cdot \alpha(1+\alpha)\,\text{Sin.}\,\theta \times \int \frac{du \,.\, c^{-u}(1 - c^{-u})}{\left\{\text{Cos.}^2\theta + 2iy\right\}^{\frac{3}{2}}}.$$

Let p denote the observed height of the mercury in the barometer reduced to the fixed temperature of 50° of Fahrenheit; τ the temperature of the air on the same scale; and $\beta = \frac{1}{480}$ the expansion for one degree: then,

$$\alpha\left(1 + \frac{\delta\alpha}{a}\right) = \frac{\alpha}{1 + \beta(\tau - 50)} \times \frac{p}{30}$$

$$i\left(1 + \frac{\delta i}{i}\right) = i\left(1 + \beta(\tau - 50)\right)$$

$$\lambda + \delta\lambda = \frac{\alpha}{i} \times \frac{1 + \frac{\delta\alpha}{a}}{1 + \frac{\delta i}{i}} = \frac{\lambda}{1 + 2\beta(\tau - 50)} \times \frac{p}{30}.$$

consequently,

$$\frac{\delta i}{i} = \frac{\tau - 50}{480}$$

$$\delta\lambda = -\frac{2\lambda}{480}(\tau - 50) - \frac{\lambda}{30}(30 - p).$$

By substituting these values, we get,

$$r = \left(1 + \frac{\delta\alpha}{\alpha}\right) \cdot \alpha(1+\alpha)\,\text{Sin.}\,\theta \times \int \frac{du \,.\, c^{-u}}{\sqrt{\text{Cos.}^2\theta + 2iy}}$$

$$-(\tau-50)\times\frac{\alpha(1+\alpha)\,\text{Sin.}\,\theta}{480}\times\left\{i\int\frac{du\,c^{-u}.y}{\{\text{Cos.}^2\,\theta+2iy\}^{\frac{3}{2}}}+2i\lambda.\int\frac{du\,c^{-u}(1-c^{-u})}{\{\text{Cos.}^2\,\theta+2iy\}^{\frac{3}{2}}}\right\}$$

$$-(30-p).\frac{\lambda.\alpha(1+\alpha)\,\text{Sin.}\,\theta}{30}.\,i\int\frac{du\,c^{-u}(1-c^{-u})}{\{\text{Cos.}^2\,\theta+2iy\}^{\frac{3}{2}}}.$$

Let us now assume

$$r=\left(1+\frac{\delta\alpha}{\alpha}\right).\,\delta\theta+(\tau-50)\frac{d\delta\theta}{d\tau}-(30-p).\frac{d\delta\theta}{dp},$$

$\delta\theta$ being the mean refraction at the apparent zenith distance θ; then, by equating the like parts of the equivalent expressions, we get,

$$\delta\theta=\alpha(1+\alpha)\,\text{Sin.}\,\theta\times\int\frac{du\,c^{-u}}{\sqrt{\text{Cos.}^2\,\theta+2iy}},$$

$$\frac{d\delta\theta}{d\tau}=-\frac{\alpha(1+\alpha)\,\text{Sin.}\,\theta}{480}\times\left\{i\int\frac{du\,c^{-u}y}{\{\text{Cos.}^2\;+2iy\}^{\frac{3}{2}}}+2i\lambda.\int\frac{du\,c^{-u}(1-c^{-u})}{\{\text{Cos.}^2\,\theta+2iy\}^{\frac{3}{2}}}\right\}$$

$$\frac{d\delta\theta}{dp}=\frac{\lambda\alpha(1+\alpha)}{30}\times\int\frac{du.c^{-u}(1-c^{-u})}{\{\text{Cos.}^2\,\theta+2iy\}^{\frac{3}{2}}};$$

each of which expressions must be separately considered.

Now we have

$$y=u+(f-\lambda).(1-c^{-u})-f(c^{-u}-1+u);$$

and if we substitute this value of y in the expression of $\delta\theta$, and then expand the radical quantity, retaining only the terms of the first order, we shall get,

$$\Delta=\sqrt{\text{Cos.}^2\,\theta+2iu}$$

$$\delta\theta=\alpha(1+\alpha)\,\text{Sin.}\,\theta\times\left\{\int\frac{du\,c^{-u}}{\Delta}-i(f-\lambda).\int\frac{du.c^{-u}(1-c^{-u})}{\Delta^3}+if.\int\frac{du.c^{-u}(c^{-u}-1+u)}{\Delta^3}\right\}.$$

It will presently be shown that the other terms of the expansion may be neglected; because $f - \lambda$ is very small; and because the function multiplied by f becomes inconsiderable after integration. Since we want only the definite integrals between the limits $u = o$ and $u = \infty$, by applying the method already used, we shall obtain,

$$\delta\theta = \alpha(1+\alpha)\,\text{Sin.}\,\theta \times \left\{ \int \frac{du\,c^{-u}}{\Delta} \right.$$
$$- (f-\lambda) . \int \frac{du}{\Delta} . \frac{d . c^{-u}(1 - c^{-u})}{du}$$
$$\left. + f . \int \frac{du}{\Delta} . \frac{d . c^{-u}(c^{-u} - 1 + u)}{du} \right\}$$

or, which is the same thing,

$$\delta\theta = \alpha(1+\alpha)\,\text{Sin.}\,\theta \times \left\{ \int \frac{du\,c^{-u}}{\Delta} \right.$$
$$- (f-\lambda) . \int \frac{du(2c^{-2u} - c^{-u})}{\Delta}$$
$$\left. + f . \int \frac{du(2c^{-u} - 2c^{-2u} - uc^{-u})}{\Delta} \right\}.$$

In order to estimate the error produced by the terms of the expansion left out, we may compare the amount of the foregoing formula at the horizon, with the exact value of the horizontal refraction already computed. Now, when $\text{Sin.}\,\theta = 1$, $\text{Cos.}\,\theta = o$, $\Delta = \sqrt{2iu}$; and the expression will become,

$$\delta\theta = \frac{\alpha(1+\alpha)}{\sqrt{2i}} \times \left\{ \int \frac{du\,c^{-u}}{\sqrt{u}} - (f-\lambda) . \int \frac{du(2c^{-2u} - c^{-u})}{\sqrt{u}} \right.$$
$$\left. + f \int \frac{du(2c^{-u} - 2c^{-2u} - uc^{-u})}{\sqrt{u}} \right\}.$$

And if we now make $u = t^2$, and then integrate between the limits $t = o$, $t = \infty$, we shall get

$$\delta\theta = \frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}} \times \left\{ 1 - (f-\lambda).(\sqrt{2} - 1) + f\left(\tfrac{3}{2} - \sqrt{2}\right) \right\}.$$

Hence $$\delta\theta = 2055''.6.$$

But we before found the horizontal refraction equal to $2058''.5$; and the difference is therefore no more than $2''.9$, which is of no moment, since the refractions just at the horizon are alone affected by the error.

In order to reduce the expression of $\delta\theta$ to the most simple form for calculation, we have,

$$\Delta c^{-u} - \text{Cos.}\,\theta = i\int\frac{du\,c^{-u}}{\Delta} - \text{Cos.}^2\theta\int\frac{du\,c^{-u}}{\Delta} - 2i\int\frac{du.uc^{-u}}{\Delta};$$

the integrals commencing when $u = o$: and when they are extended to $u = \infty$, we get

$$\int\frac{du.uc^{-u}}{\Delta} = \frac{1}{2}\int\frac{du.c^{-u}}{\Delta} - \frac{\text{Cos.}^2\theta}{2i}\int\frac{du\,c^{-u}}{\Delta} + \frac{\text{Cos.}\,\theta}{2i}.$$

Now put

$$\text{N} = \frac{\text{Cos.}^2\theta}{2i}\int\frac{du\cdot c^{-u}}{\Delta} - \frac{\text{Cos.}\,\theta}{2i}$$

$$\text{M} = 2\int\frac{du\,c^{-2u}}{\Delta} - \int\frac{du\,c^{-u}}{\Delta};$$

then by substitution we shall finally get

$$\delta\theta = \alpha\,(1+\alpha)\,\text{Sin.}\,\theta \times \left\{\int\frac{du\,c^{-u}}{\Delta} + \lambda\,.\,\text{M} - f\left(2\,\text{M} - \frac{1}{2}\int\frac{du\,c^{-u}}{\Delta} - \text{N}\right)\right\}.$$

The expression of $\frac{d\delta\theta}{d\tau}$ may be put in this form, viz.

$$\frac{d\delta\theta}{d\tau} = -\frac{\alpha\,(1+\alpha)\,\text{Sin.}\,\theta}{480}\cdot\left\{\frac{1}{2}\cdot\int\frac{du\,c^{-u}}{\sqrt{\text{Cos.}^2\theta + 2iy}} - \frac{\text{Cos.}^2\theta}{2}\cdot\int\frac{du\,c^{-u}}{\left\{\text{Cos.}^2\theta + 2iy\right\}^{\frac{3}{2}}} + 2i\lambda\cdot\int\frac{du\,c^{-u}\,(1-c^{-u})}{\left\{\text{Cos.}^2\theta + 2iy\right\}^{\frac{3}{2}}}\right\}:$$

and it will be sufficiently accurate to write u for y in the denominators of the two last terms: then

$$\frac{d\delta\theta}{d\tau} = -\frac{1}{480}\cdot\frac{\delta\theta}{2} + \frac{\alpha(1+\alpha)\,\text{Sin.}\,\theta}{480}\cdot\left\{i \times 2\lambda\int\frac{duc^{-u}(1-c^{-u})}{\Delta^3} - \frac{\text{Cos.}^2\,\theta}{2}\cdot\int\frac{duc^{-u}}{\Delta^3}\right\}:$$

and, by the same procedure as before,

$$\frac{d\delta\theta}{d\tau} = -\frac{1}{480}\cdot\frac{\delta\theta}{2} + \frac{\alpha(1+\alpha)\,\text{Sin.}\,\theta}{480}\cdot\left\{2\lambda\text{M} - \frac{\text{Cos.}^2\,\theta}{2}\int\frac{duc^{-u}}{\Delta^3}\right\}.$$

But we have

$$\frac{1}{\text{Cos.}\,\theta} - \frac{c^{-u}}{\Delta} = \int\frac{duc^{-u}}{\Delta} + i\int\frac{duc^{-u}}{\Delta^3},$$

the integrals commencing when $u = o$; and, when $u = \infty$, we get,

$$\frac{\text{Cos.}^2\,\theta}{2}\cdot\int\frac{duc^{-u}}{\Delta^3} = -\frac{\text{Cos.}^2\theta}{2i}\int\frac{duc^{-u}}{\Delta} + \frac{\text{Cos.}\,\theta}{2i} = -\text{N}.$$

Wherefore,

$$\frac{d\delta\theta}{d\tau} = -\frac{1}{480}\cdot\frac{\delta\theta}{2} + \frac{\alpha(1+\alpha)\,\text{Sin.}\,\theta}{480}\cdot\left\{2\lambda\text{M} + \text{N}\right\}:$$

and, by substituting the value of $\delta\theta$,

$$\frac{d\delta\theta}{d\tau} = -\frac{\alpha(1+\alpha)\,\text{Sin.}\,\theta}{480}\times\left\{\frac{1}{2}\int\frac{duc^{-u}}{\Delta} + \frac{5}{2}\lambda\text{M} + \text{N} - \frac{f}{2}\left(2\text{M} - \frac{1}{2}\int\frac{duc^{-u}}{\Delta} - \text{N}\right)\right\}.$$

Lastly by writing u for y in the expression of $\frac{d\delta\theta}{dp}$, we readily obtain

$$\frac{d\delta\theta}{dp} = +\frac{\alpha(1+\alpha)\,.\,\lambda\,\text{Sin.}\,\theta}{30}\times\text{M}.$$

Thus the quantities $\delta\theta$, $\frac{d\delta\theta}{d\tau}$, $\frac{d\delta\theta}{dp}$, ultimately involve only two different integrals, viz. $\int\frac{duc^{-u}}{\Delta}$ and $\int\frac{duc^{-2u}}{\Delta}$; the values of which we must next endeavour to investigate.

13. The whole integral $\int \frac{du\,c^{-u}}{\sqrt{\text{Cos.}^2\theta + 2iu}}$, extending from $u = o$ to $u = \infty$, is composed of these two parts, viz.

$$\int \frac{du\,c^{-u}}{\sqrt{\text{Cos.}^2\theta + 2iu}} + c^{-m} . \int \frac{du\,c^{-u}}{\sqrt{\text{Cos.}^2\theta + 2im + 2iu}};$$

the first part being contained between the limits $u = o$, and $u = m$; and the second part, which arises from substituting $m + u$ for u in the first part, being extended from $u = o$ to $u = \infty$.

To begin with the first part: put

$$u = m\,(1 - e^2)\,z + m\,e^2 z^2;$$

and the limits of u being o and m, the limits of z will be o and 1. In order to determine e, assume

$$\Delta = \sqrt{\text{Cos.}^2\theta + 2im(1 - e^2)z + 2ime^2z^2} = \text{Cos.}\,\theta + ez\sqrt{2im}:$$

then

$$\frac{\sqrt{2im}}{\text{Cos.}\,\theta} = \frac{2e}{1 - e^2},$$

$$\frac{du}{\Delta} = \frac{2e}{\sqrt{2im}} \times m\,dz,$$

$$\int \frac{du\,c^{-u}}{\Delta} = \frac{2e}{\sqrt{2im}} \times \int m\,dz\,.\,c^{-mz + e^2 m\,(z - z^2)}.$$

Let the integral sought, viz. $\int \frac{du\,c^{-u}}{\Delta}$ between the limits $u = o$, $u = m$, be expressed in a series of this form, viz.

$$\int \frac{du\,c^{-u}}{\Delta} = \frac{2}{\sqrt{2im}} \times \left\{ \text{A}^{(o)}.\,e + \text{A}^{(1)} e^3 \ldots\ldots + \text{A}^{(n)} e^{2n+1} \ldots \ \&\text{c.} \right\}$$

then if we develope the foregoing exponential value in a series of the powers of e, and equate the like terms of the equivalent expressions, we shall get,

$$\text{A}^{(n)} = \frac{m^{n+1}}{1\,.\,2\,.\,3 \ldots\ldots n} \cdot \int dz\,(z - z^2)^n c^{-mz},$$

the integral being taken between the limits $z = o$, $z = 1$.

But the integral between the limits mentioned, is equal to the difference of the two values of the same integral taken, the one between the limits $z = o$, $z = \infty$; and the other between the limits $z = 1$, $z = \infty$. Now, by writing $1 + z$ for z, the expression

$$\int dz\,(z - z^2)^n c^{-mz},$$

will be changed into

$$(-1)^n . c^{-m} . \int dz\,(z + z^2)^n c^{-mz};$$

and it is obvious that the value of the former between the limits $z = 1$, $z = \infty$, is equal to the value of the latter between the limits $z = o$, $z = \infty$. It follows therefore from what has been said, that we shall have

$$A^{(n)} = \frac{m^{n+1}}{1 . 2 . 3 \ldots n} \times \left\{ \int dz\,(z - z^2)^n c^{-mz} - (-1)^n . c^{-m} . \int dz\,(z + z^2)^n c^{-mz} \right\},$$

each of the integrals being extended from $z = o$ to $z = \infty$.

Again, p being any whole number, we have, between the limits $z = o$, $z = \infty$,

$$\int dz . z^p . c^{-mz} = \frac{1 . 2 . 3 \ldots p}{m^{p+1}}.$$

Wherefore if we expand the binomial quantities in the value of $A^{(n)}$, and integrate the terms separately, we shall obtain

$$A^{(n)} = 1 - n . \frac{n+1}{m} + n . \frac{n-1}{2} . \frac{n+1 . n+2}{m^2} - n . \frac{n-1}{2} . \frac{n-2}{3} . \frac{n+1 . n+2 . n+3}{m^3} + \&c.$$
$$- (-1)^n . c^{-m} . \left(1 + n . \frac{n+1}{m} + n . \frac{n-1}{2} . \frac{n+1 . n+2}{m^2} + \&c.\right).$$

By this means we get the first part of the integral sought in a series that has all its terms positive, and that will always converge because *e* never exceeds unit.

Let us next consider the second, or supplemental part, viz.

$$c^{-m} \times \int \frac{du\,c^{-u}}{\sqrt{\text{Cos.}^2\theta + 2im + 2iu}}.$$

Now $\text{Cos.}^2\,\theta = \frac{(1-e^2)^2}{4e^2} \times 2im$: and, if we substitute this value, the integral will become

$$\frac{2}{\sqrt{2im}} \times \frac{c^{-m}}{2} \times \frac{2e}{1+e^2} \cdot \int \frac{du\,c^{-u}}{\sqrt{1+\left(\frac{2e}{1+e^2}\right)^2 \cdot \frac{u}{m}}}:$$

and, by expanding the denominator and integrating between the limits $u = 0$, $u = \infty$, we shall get this value of the quantity sought, viz.

$$\begin{aligned}\frac{2}{\sqrt{2im}} \cdot \frac{c^{-m}}{2} \times \Bigg\{ & \frac{2e}{1+e^2} - \frac{1}{m} \cdot \frac{1}{2^2}\left(\frac{2e}{1+e^2}\right)^3 \\ & + \frac{1 \cdot 3}{m^2} \cdot \frac{1}{2^4} \cdot \left(\frac{2e}{1+e^2}\right)^5 \\ & - \frac{1 \cdot 3 \cdot 5}{m^3} \cdot \frac{1}{2^6}\left(\frac{2e}{1+e^2}\right)^7 \\ & + \&\text{c.} \Bigg\}\end{aligned}$$

This series will converge in its first terms: and the results being alternately too great and too small, we can thus estimate the degree of approximation.

By uniting the two parts, we get this expression for the whole integral between the limits $u = 0$, $u = \infty$, m being an arbitrary quantity, viz.

$$\begin{aligned}\int \frac{du\,c^{-u}}{\Delta} = {} & \frac{2}{\sqrt{2im}} \cdot \left\{ \mathrm{A}^{(0)} e + \mathrm{A}^{(1)} e^3 + \mathrm{A}^{(2)} e^5 + \&\text{c.} \right\} \\ & + \frac{2}{\sqrt{2im}} \times \frac{c^{-m}}{2} \times \Bigg\{ \frac{2e}{1+e^2} - \frac{1}{m} \cdot \frac{1}{2^2}\left(\frac{2e}{1+e^2}\right)^3 \\ & \qquad + \frac{1 \cdot 3}{m^2} \cdot \frac{1}{2^4}\left(\frac{2e}{1+e^2}\right)^5 \\ & \qquad - \frac{1 \cdot 3 \cdot 5}{m^3} \cdot \frac{1}{2^6} \cdot \left(\frac{2e}{1+e^2}\right)^7 \\ & \qquad + \&\text{c.} \Bigg\}\end{aligned}$$

The supplemental part is less than

$$\frac{2}{\sqrt{2im}} \times \frac{e c^{-m}}{1 + e^2}:$$

it is therefore very small when m is a considerable number, in which case the value of the integral will be found with sufficient exactness by means of the first series alone. But, with regard to the foregoing expression, we must not omit to remark what is a very curious instance of the artifices that must sometimes be resorted to in order to bring an analytical expression within the boundary of arithmetical computation. If the supplemental part be developed in a series of the powers of e, it will consist of precisely the same terms, but with opposite signs, as that part of the first series which is multiplied by c^{-m}. In reality, therefore, the exact value of the integral is what remains of the first series, when the part multiplied by c^{-m} is thrown out; which is also very manifest from the mode of investigation. But the series so obtained is imperfectly computable. It belongs to that class called semi-convergent; which converge indeed to a certain degree in their first terms, but afterwards become divergent. By adding and subtracting the same quantity in two different shapes, an expression is produced consisting of two parts, that can be calculated separately to any degree of exactness.

For the sake of brevity, let the supplemental part be represented by $\frac{2}{\sqrt{2im}} \cdot c^{-m} \cdot \mathrm{R}$: then, if we separate from the first series the part of $\mathrm{A}^{(0)}$ multiplied by c^{-m}, we shall have

$$\int \frac{du\, c^{-u}}{\Delta} = \frac{2}{\sqrt{2im}} \times \left\{ e + \mathrm{A}^{(1)} e^3 + \mathrm{A}^{(2)} e^5 + \&c. \right\}$$

$$+ \frac{2 c^{-m}}{\sqrt{2im}} \times (\mathrm{R} - e):$$

And it follows, from what has been said, that the subsidiary part of this expression is no other than the expansion of R deprived of its first term. In like manner if we separate the parts of $A^{(0)}$, $A^{(1)}$, $A^{(2)}$ which involve c^{-m}, we shall get

$$\int \frac{duc^{-u}}{\Delta} = \frac{2}{\sqrt{2im}} \times \left\{ e + \left(1 - \frac{2}{m}\right) e^3 + \left(1 - \frac{6}{m} + \frac{12}{m^2}\right) e^5 + A^{(3)} e^7 + \&c. \right\}$$

$$+ \frac{2c^{-m}}{\sqrt{2im}} \cdot \left\{ R - e + \left(1 + \frac{2}{m}\right) e^3 - \left(1 + \frac{6}{m} + \frac{12}{m^2}\right) e^5 \right\};$$

And here the subsidiary part is the expansion of R wanting the three first terms. On account of the factor c^{-m}, the subsidiary parts decrease without limit as m increases; and thus the value of the integral can always be found to any required degree of exactness, in a series coinciding with the rigorous expression in its first terms, at the same time that it converges in its remaining terms.

Now, let $m = 8$: then

$$A^{(3)} = \frac{13}{64} + \frac{235}{64} c^{-8} = 0.204357$$

$$A^{(4)} = \frac{21}{256} - \frac{2141}{256} c^{-8} = 0.079225$$

$$A^{(5)} = \frac{19}{1024} + \frac{23029}{1024} c^{-8} = 0.026099$$

$$A^{(6)} = \frac{127}{4096} - \frac{287575}{4096} c^{-8} = 0.007453$$

$$A^{(7)} = -\frac{1347}{16384} + \frac{4106939}{16384} c^{-8} = 0.001876$$

$$A^{(8)} = \frac{22237}{65536} - \frac{66205285}{65536} c^{-8} = 0.000422$$

And hence, neglecting the parts of $A^{(0)}$, $A^{(1)}$, $A^{(2)}$, that involve c^{-m}, we get

$$\frac{4\sqrt{i}}{\text{Cos. } \theta} = \frac{2e}{1 - e^2}$$

$$\int \frac{du\,c^{-u}}{\Delta} = \frac{1}{2\sqrt{i}} \times \Big\{ e + \frac{3}{4}e^3 + \frac{7}{16}e^5 + 0.204357.e^7 + 0.079225.e^9 + 0.026099.e^{11} + 0.007453.e^{13} + 0.001876.e^{15} + 0.000422.e^{17}$$

Although we are sure that this value is a near approximation to the truth, yet it may not be superfluous to examine whether it be sufficiently exact for the purpose intended. Now, the part of $\delta\theta$ depending on this integral, is

$$\alpha\,(1 + \alpha)\,\text{Sin.}\,\theta\,.\int \frac{du\,c^{-u}}{\Delta}:$$

and, this being valued by means of the foregoing series in the case of Sin. $\theta = 1$, and $e = 1$, in which circumstances the error of the approximation is greatest, the result will be

$$\frac{\alpha\,(1+\alpha)}{2\sqrt{i}} \times 2.506932 = 2038''.2\,.$$

But, when Sin. $\theta = 1$, $\Delta = \sqrt{2\,iu}$; and the quantity we are considering will become

$$\frac{\alpha\,(1+\alpha)}{\sqrt{2\,i}}\,.\int \frac{du\,c^{-u}}{\sqrt{u}}:$$

and if we put $u = t^2$, and integrate between the limits $t = o$, $t = \infty$, the exact value will be equal to

$$\frac{\alpha\,(1+\alpha)\,\sqrt{\pi}}{\sqrt{2\,i}} = 2037''.8\,.$$

It appears, therefore, that the error of the approximation when it is greatest, or when $e = 1$, does not amount to half a second. But as the error is expressed by a series of terms multiplied by e^7, e^9, &c. it diminishes very rapidly as e decreases, and becomes altogether insensible when θ is less than 90°.

It remains to find the value of the integral $\int \frac{du\,c^{-2u}}{\Delta}$ between the limits $u=o$, $u=\infty$. By substitution we get

$$2\int \frac{du\,c^{-2u}}{\Delta} = \frac{2e}{\sqrt{2im}} \times \int 2\,m\,dz.c^{-2mz+2me^2(z-z^2)}$$

from which it is manifest that we shall obtain the value sought by substituting $2m$ in place of m in the coefficients of the former series; thus

$$2\int \frac{du\,c^{-2u}}{\Delta} = \frac{2}{\sqrt{2im}} \times \left\{ e + \left(1-\frac{2}{2m}\right)e^3 + \left(1-\frac{6}{2m}+\frac{12}{4m^2}\right)e^5 \right.$$
$$\left. + A'^{(3)}e^7 + A'^{(4)}e^9 + \&c. \right\},$$

$A'^{(3)}$, $A'^{(4)}$, &c. denoting what $A^{(3)}$, $A^{(4)}$, &c. become when $2m$ is substituted for m. Hence, making $m=8$, $2m=16$, we get

$$A'^{(3)} = \frac{233}{512} + \frac{1031}{512}c^{-16} = 0.455078.$$

$$A'^{(4)} = \frac{1121}{4096} - \frac{13041}{4096}c^{-16} = 0.273681$$

$$A'^{(5)} = \frac{4823}{32768} + \frac{183353}{32768}c^{-16} = 0.147188$$

$$A'^{(6)} = \frac{18691}{262144} - \frac{2851507}{262144}c^{-16} = 0.071299$$

$$A'^{(7)} = \frac{65689}{2097152} + \frac{48804183}{2097152}c^{-16} = 0.031326$$

$$A'^{(8)} = \frac{210889}{16777216} - \frac{914559193}{16777216}c^{-16} = 0.012564$$

Wherefore,

$$2\int \frac{du\,c^{-2u}}{\Delta} = \frac{1}{2\sqrt{i}} \times \left\{ e + \frac{3}{8}e^3 + \frac{43}{64}e^5 + 0.455078.e^7 \right.$$
$$+ 0.273681.e^9$$
$$+ 0.147188.e^{11}$$
$$+ 0.071299.e^{13}$$
$$+ 0.031326.e^{15}$$
$$+ 0.012564.e^{17}$$

Having now found the values of the two integrals on which the expression of the refraction depends, we get immediately

$$\begin{aligned} M = \frac{1}{2\sqrt{i}} \times \Big\{ & \tfrac{1}{8} \cdot e^2 \\ & + 0.234375.e^5 \\ & + 0.250721.e^7 \\ & + 0.194456.e^9 \\ & + 0.121089.e^{11} \\ & + 0.063846.e^{13} \\ & + 0.029450.e^{15} \\ & + 0.012142.e^{17} \end{aligned} \qquad \begin{aligned} \lambda . M = \frac{1}{2\sqrt{i}} \times \Big\{ & 0.027386.e^3 \\ & + 0.051349.e^5 \\ & + 0.054930.e^7 \\ & + 0.042602.e^9 \\ & + 0.026529.e^{11} \\ & + 0.013987.e^{13} \\ & + 0.006451.e^{15} \\ & + 0.002659.e^{17} \end{aligned}$$

And again, we have

$$N = \frac{\text{Cos.}^2\,\theta}{2\,i} \int \frac{du\,c^{-u}}{\Delta} - \frac{\text{Cos.}\,\theta}{2\,i}:$$

but

$$\frac{\text{Cos.}^2\,\theta}{2\,i} = \frac{m}{4} \cdot \frac{(1-e^2)^2}{e\,2} = 2 \cdot \frac{(1-e^2)^2}{e^2}; \; \frac{\text{Cos.}\,\theta}{2\,i} = \frac{1}{2\sqrt{i}} \times 2 \cdot \frac{1-e^2}{e}:$$

and hence, if we put $\int \frac{du\,c^{-u}}{\Delta} = \frac{1}{2\sqrt{i}} \times \psi$; so that ψ stands for the series in the value of the integral; we shall get

$$N = \frac{1}{2\sqrt{i}} \times 2 \left\{ \frac{(1-e^2)^2}{e^2} \cdot \psi - \frac{1-e^2}{e} \right\}.$$

Wherefore, by substituting the value of ψ, viz.

$$\psi = e + \tfrac{3}{4} e^3 + \tfrac{7}{16} e^5 + A^{(3)} . e^7 + A^{(4)} . e^9 + \&\text{c.}$$

we shall find

$$\begin{aligned} N = \frac{1}{2\sqrt{i}} \times \Big\{ -\tfrac{1}{2} e - \tfrac{1}{8} e^3 & + 2\,(\tfrac{3}{4} - \tfrac{7}{8} + A^{(3)}) . e^5 \\ & + 2\,(\tfrac{7}{16} - 2\,A^{(3)} + A^{(4)}) . e^7 \\ & + 2\,(A^{(3)} - 2\,A^{(4)} + A^{(5)}) . e^9 \\ & + 2\,(A^{(4)} - 2\,A^{(5)} + A^{(6)}) . e^{11} \\ & + 2\,(A^{(5)} - 2\,A^{(6)} + A^{(7)}) . e^{13} \\ & + 2\,(A^{(6)} - 2\,A^{(7)} \quad * \quad) . e^{15} \\ & + 2\,(A^{(7)} \quad * \quad * \quad) . e^{17} \end{aligned}$$

And, in numbers,

$$\begin{aligned} N = \frac{1}{2\sqrt{i}} \times \Big\{ -\tfrac{1}{2}e - \tfrac{1}{8}e^3 & + 0.158714 . e^5 \\ & + 0.216022 . e^7 \\ & + 0.144012 . e^9 \\ & + 0.068960 . e^{11} \\ & + 0.026138 . e^{13} \\ & + 0.007402 . e^{15} \\ & + 0.003752 . e^{17} \end{aligned}$$

To find $\delta\theta$, it only remains to substitute the numerical values of $\int \frac{duc^{-u}}{\Delta}$, λ M, and N, in the expression investigated in No. 12; then,

$$\begin{aligned} \delta\theta = \frac{\alpha(1+\alpha)}{2\sqrt{i}} \times \text{Sin.}\,\theta \times e \times \Big\{ 1 & + 0.777386 . e^2 \\ & + (0.488849 - 0.091286 . f) . e^4 \\ & + (0.259287 - 0.183242 . f) . e^6 \\ & + (0.121827 - 0.205287 . f) . e^8 \\ & + (0.052628 - 0.160168 . f) . e^{10} \\ & + (0.021440 - 0.097828 . f) . e^{12} \\ & + (0.008327 - 0.050560 . f) . e^{14} \\ & + (0.003081 - 0.020321 . f) . e^{16} \end{aligned}$$

The two first terms of this expression do not contain f; and they give that part of the refractions near the zenith, which has no dependence upon the constitution of the atmosphere. As there is some uncertainty in the value of f, it may be determined either so as to make the horizontal refraction coincide with the quantity adopted by astronomers; or so as to make the formula represent some very exact observations made at low altitudes, from 2° to 7° above the horizon. With regard to altitudes less than 2°, it is not clear that the astronomical refractions do not participate of the extreme irregularity that attends the terrestrial refractions,

which would render such observations unfit to be employed in this research. But in the present state of our knowledge it may be doubted, whether a more satisfactory determination of f can be obtained than what we have hitherto assumed, namely, $f = \frac{1}{4}$. With this value, we get

$$\begin{aligned}\delta\theta = \frac{\alpha(1+\alpha)}{2\sqrt{i}} \times \text{Sin.}\,\theta \times e \times \Big\{ 1 &+ 0.777386 \,.\, e^2 \\ &+ 0.466028 \,.\, e^4 \\ &+ 0.213477 \,.\, e^6 \\ &+ 0.070505 \,.\, e^8 \\ &+ 0.012586 \,.\, e^{10} \\ &- 0.003017 \,.\, e^{12} \\ &- 0.004313 \,.\, e^{14} \\ &- 0.001999 \,.\, e^{16}\end{aligned}$$

$$\text{Log.}\ \frac{\alpha(1+\alpha)}{2\sqrt{i}} = 2.9101040.$$

$$\text{Tan.}\ \varphi = 19.1580271 + \text{Sec.}\ \theta - 20;\quad e = \text{Tan.}\ \tfrac{1}{2}\varphi.$$

If we make $\theta = 90^\circ$, and $e = 1$, we get,

$$\delta\theta = \frac{\alpha(1+\alpha)}{2\sqrt{i}} \times 2.530653 = 2057''.5.$$

This is the horizontal refraction by the formula: and as the exact value of the same quantity was before found equal to 2058″.5, the error arising from the method of approximation amounts only to 1″ at the horizon. But all the quantities neglected being of the orders e^7, e^9, &c. the error will be altogether insensible unless when e is extremely near 1, that is, at very low altitudes.

The foregoing expression may be put in another form, which, in some cases, is more convenient for calculation.

Since $\frac{2e}{1-e^2} = \frac{4\sqrt{i}}{\text{Cos.}\,\theta}$; we get $\frac{e}{2\sqrt{i}} = \frac{1-e^2}{\text{Cos.}\,\theta}$:

and hence, by substitution,

$$\delta\theta = \alpha\,(1+\alpha)\,\text{Tan.}\,\theta \times \left\{ \begin{array}{l} 1 - 0.222614\,.\,e^{2} \\ \quad - 0.311358\,.\,e^{4} \\ \quad - 0.252551\,.\,e^{6} \\ \quad - 0.142972\,.\,e^{8} \\ \quad - 0.057919\,.\,e^{10} \\ \quad - 0.015603\,.\,e^{12} \end{array} \right.$$

$$\text{Log.}\,\alpha\,(1+\alpha) = 1\,.\,7671011.$$

This transformation can be of use only to a certain distance from the zenith; for at the horizon Tan. θ is infinite, and the factor $1 - e^2$ is equal to zero. The expression set down is sufficient for finding the refractions exact to $\frac{1}{100}$ of a second as far as 85° from the zenith.

And, if we take the logarithms of both sides of the last expression, we shall get

$$\begin{array}{rl} \text{Log.}\,\delta\theta = \text{Log. Tan.}\,\theta & + 1\,.\,76710 \\ & - 0.096680\,.\,e^{2} \\ & - 0.145982\,.\,e^{4} \\ & - 0.141413\,.\,e^{6} \\ & - 0.114530\,.\,e^{8} \\ & - 0.089474\,.\,e^{10} \\ & - 0.073278\,.\,e^{12} \end{array}$$

which formula is very convenient near the zenith, and is sufficient for finding the logarithms of the refractions exact to five figures, as far as 84° from the zenith. It is to be observed, that while θ increases from zero, e increases from a limit, from which it varies very little till θ becomes a considerable arc.

In order to have $\frac{d\delta\theta}{d\tau}$ and $\frac{d\delta\theta}{dp}$ it is only requisite to substi-

tute the numerical values already found in the expressions investigated in No. 12 : thus,

$$
\begin{aligned}
\frac{d\,\delta\,\theta}{d\,\tau} = -\frac{\alpha\,(1+\alpha)}{2\sqrt{i}} \times \frac{1}{480} \times \text{Sin.}\,\theta \times e \times \Big\{\, & 0\,.\,31846\,.\,e^{2} \\
& + 0\,.\,49442\,.\,e^{4} \\
& + 0\,.\,43262\,.\,e^{6} \\
& + 0\,.\,26447\,.\,e^{8} \\
& + 0\,.\,12831\,.\,e^{10} \\
& + 0\,.\,05260\,.\,e^{12} \\
& + 0\,.\,01815\,.\,e^{14} \\
& + 0\,.\,00807\,.\,e^{16}
\end{aligned}
$$

$$
\text{Log.}\,\frac{\alpha\,(1+\alpha)}{2\sqrt{i}} \times \frac{1}{480} = 0\,.\,2288628.
$$

$$
\begin{aligned}
\frac{d\,\delta\,\theta}{d\,p} = \frac{\alpha\,(1+\alpha)}{2\sqrt{i}} \times \frac{\lambda}{30} \times \text{Sin.}\,\theta \times e \times \Big\{\, & 0\,.\,125\,.\,e^{2} \\
& + 0\,.\,23437\,.\,e^{4} \\
& + 0\,.\,25072\,.\,e^{6} \\
& + 0\,.\,19446\,.\,e^{8} \\
& + 0\,.\,12109\,.\,e^{10} \\
& + 0\,.\,06385\,.\,e^{12} \\
& + 0\,.\,02945\,.\,e^{14} \\
& + 0\,.\,01214\,.\,e^{1}
\end{aligned}
$$

$$
\text{Log.}\,\frac{\alpha\,(1+\alpha)}{2\sqrt{i}} \times \frac{\lambda}{30} = 0\,.\,7736018.
$$

By means of the foregoing formulæ the table annexed to this paper was computed. In the first column are placed the distances from the zenith : the second contains the values of $\delta\,\theta$, or the mean refractions at the temperature of 50° of FAHRENHEIT and the barometric pressure 30 English inches : the third contains the logarithms of the refractions : and, when the zenith distance is greater than 75°, the values of $\frac{d\,\delta\,\theta}{d\,\tau}$ and $\frac{d\,\delta\,\theta}{d\,p}$ are added in two other columns.

The use of this table will be clear, from the subjoined formula for computing by it the true refraction, supposing that τ is the temperature by Fahrenheit's thermometer, and p the height of the barometer in English inches.

$$r = \frac{1}{1+\beta(\tau-50)} \times \frac{p}{30} \times \delta\theta + \frac{d\delta\theta}{d\tau}(\tau-50) - \frac{d\delta\theta}{dp}(30-p).$$

The first term is the mean refraction corrected for the observed temperature and pressure in the same manner usually practised by astronomers. When the zenith distance does not exceed 75°, the two remaining terms are to be accounted as evanescent; and, even when the zenith distance is 80° or a little more, the same terms may, on most occasions, be omitted: otherwise the two terms, amounting generally to some seconds, are to be added to the first term with their proper signs.

Three subsidiary Tables are added for facilitating the corrections for the barometer and thermometer. Table II. contains the logarithms of $\frac{1}{1+\beta(\tau-50)} = \frac{1}{1+\frac{\tau-50}{480}}$, for 40° on either side of the mean temperature 50°; negative indices being avoided by substituting the arithmetical complements. Table III. contains the logarithms, or the arithmetical complements, of $\frac{p}{30}$ for the values of p between 31 and $27\frac{1}{2}$. Table IV. contains the small corrections, positive or negative, to be applied to the numbers in Table III. in order to reduce the observed length of the barometric column to the mean temperature of 50°. The numbers of this Table are the logarithms of $\frac{1}{1+\frac{\tau-50}{10000}}$, equal to $-\frac{\tau-50}{10000} \times \cdot 434$.

14. Instead of applying the new Tables to particular in-

stances, it will be more compendious to compare it with other tables that have been long in the hands of astronomers, and the characters of which are well established. The table has been constructed with the same elementary quantities as the French table, at least as far as regards the refractive power and the weight of the air, which quantities alone influence the magnitude of the refractions near the zenith. But, in comparing the two tables, an allowance must be made for the difference of the standard barometers; and this requires that the refractions in the new table be all diminished by $\frac{8}{3000} = \frac{1}{375}$. Now, taking the refractions at 45° and 80° from the zenith, we get

$$58''.36\left(1 - \frac{1}{375}\right) = 58''.2,$$

$$320.19\left(1 - \frac{1}{375}\right) = 319''.3;$$

and, in the French table, we find the first of these numbers exactly, and 319″.8 in place of the second. But we must not forget that there is a small subtractive correction to be applied to the mean refractions of the French astronomers, which is usually neglected, although it will be found among the tables of refraction (Table V.) inserted in the *Tables Astronomiques*, published by them in 1806. This correction amounts to 0″.5 at 80° from the zenith; and the former number is thus reduced to 319″.3, the same as in the new table. We may therefore conclude, that when we calculate rigorously, the mean refractions of the new table are the same as those of the French astronomers, as far as 81° or 82° from the zenith.

But at lower altitudes there will no longer be the same

perfect agreement between the two tables: first, on account of the difference in the hypotheses respecting the constitution of the atmosphere; secondly, because the tables are differently constructed.

The French refractions at low altitudes are computed by a formula which the sagacity of Laplace deduced from a hypothesis respecting density, that must be a near approach to the law that actually obtains in nature. The formula is so constructed as to give the horizontal refraction adopted by astronomers; but we may still judge, in some measure, of the accuracy of the hypothesis, by comparing the rate of the decrease of heat at the earth's surface with the result of actual observation. Now, in the hypothesis of Laplace, I have found

$$\frac{d\omega}{ds}(\text{making } s = o) = 0.7159\,;$$

which is a near approach to 0.8, the quantity assumed in this Paper. But the difference, although it seem very little, has nevertheless a great influence on the constitution of the atmosphere, as will be obvious if, by means of the equation

$$\frac{d\omega}{ds}(s = o) = 1 - \frac{\beta \times l}{\mu},$$

we compute the value of μ resulting from the preceding value of $\frac{d\omega}{ds}$. It will be found that $\mu = 59\frac{1}{2}$; which is the elevation in fathoms that in this hypothesis will depress the centigrade thermometer one degree; and it is no more than about $\frac{2}{3}$ of the true quantity. It follows, therefore, that the theory of Laplace does not strictly accord with the actual condition of the atmosphere, which must affect the accuracy of the refractions near the horizon.

The French tables are also liable to some inaccuracy at low altitudes, from the manner in which they are constructed. The calculations are originally made for the temperature zero of the centigrade scale, and the barometric pressure 29.92 English inches; and from the numbers so computed, the refractions are in every case deduced, on the supposition that they vary in the same proportion as the density of the air. But, besides this alteration of their quantity, the refractions undergo other variations, as the elementary quantities of the formula change with the state of the air; namely, those contained in the second and third terms of the foregoing rule for calculating by the new table. Now, the variations here alluded to are neglected in the French table, although they are of considerable amount near the horizon. They are neglected, however, not because the eminent mathematicians who constructed the table were not aware of their existence, but because they deemed them of little moment in a case of so great uncertainty as the refractions at low altitudes. Properly speaking, the table in the *Connaissance des Tems* is not one of *mean refractions;* that is, the numbers in it are not the same that would be found by substituting, in the fundamental formula, the elementary quantities reduced to the proposed standards of temperature and pressure. The true mean refractions computed in this manner would all be less than the quantities actually contained in the table. In practice, therefore, there is a kind of compensation that takes place between the excess of the numbers in the table above the exact values of the mean refractions, and the manner of correcting for the barometer and thermometer; a compensation which is very happy in many

instances, but which cannot fail to leave a final error in a numerous set of observations.

We are in possession of another table of refractions, computed with great care, published in 1818, by M. Bessel, in his *Astronomiæ Fundamenta.* This table must be considered as having the authority of actual observation as far as 86° from the zenith; since, to that extent, it represents with great exactness the observations of Dr. Bradley, which served as the basis of its construction. At lower altitudes, the refractions in it are confessedly too great. To compare the new table with that of M. Bessel is, therefore, the same as to make a direct appeal to experience.

The astronomical refractions were first discussed with a due attention to all the circumstances of the problem in the Treatise published by Kramp.* This author gives the name of specific elasticity to the quotient of the relative pressure divided by the relative density of the air; it is therefore equal to $\frac{1+\beta\tau}{1+\beta\tau'}$ in the formulæ of the present paper. He represents it by $c^{-\varepsilon s}$, ε being a small fraction; which function, therefore, contains the law for the gradation of heat according to Kramp. Now, if we substitute $c^{-\varepsilon s}$ for $\frac{1+\beta\tau}{1+\beta\tau'}$ in the foregoing formulæ, and then equate the two values of P, we shall get

$$(1-\omega)\times c^{-\varepsilon s} = \int - ds\,(1-\omega);$$

and hence

$$1-\omega = c^{-\frac{1}{\varepsilon}(c^{\varepsilon s}-1)+\varepsilon s}.$$

This is the rigorous expression of the density in the hypo-

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* Analyse des Refractions Astronomiques et Terrestres, 1798.

thesis of KRAMP; but, as it is too complicated for calculation, he deduces from it this more simple value, viz.

$$1 - \omega = c^{-(1-\varepsilon)s}$$

by retaining only the part of the expansion of the function in the index that contains the first power of s.

In all this KRAMP is followed by BESSEL, whose aim is to determine the value of ε that will best represent all the observations of Dr. BRADLEY, without paying any regard to the terrestrial phenomena, or to any farther theoretical considerations whatever.

Now, there is an essential distinction between the rigorous expression of the density, and the approximate value used instead of it. The latter belongs to a finite atmosphere, and the former to one of unlimited extent. To prove this, we need only substitute $c^{-(1-\varepsilon)s}$ for $1 - \omega$ in the equation,

$$P = \int - ds\,(1 - \omega);$$

and then we shall get

$$P = \frac{c^{-(1-\varepsilon)s}}{1-\varepsilon} - \frac{\varepsilon}{1-\varepsilon};$$

the constant quantity being necessary, because $P = 1$ when $s = 0$. But as P cannot be negative, we have $P = 0$ at the top of the atmosphere; and the total height will therefore be determined by the equation

$$c^{-(1-\varepsilon)s} - \varepsilon = 0.$$

If we could suppose that ε is a very small fraction, and the height of the atmosphere very great, what has just been observed would be of little consequence. But, at the surface of the earth, we ought to have $\frac{d\omega}{ds} = 1 - \varepsilon = \frac{4}{5}$, and $\varepsilon = \frac{1}{5}$; which would limit the atmosphere to about double the height

in the hypothesis of CASSINI. BESSEL determines $\varepsilon = \frac{1}{28}$ nearly; which is quite inconsistent with the value of $\frac{d\omega}{ds}$ at the surface of the earth, and with the elevation necessary for depressing the thermometer one degree, as found by experiment. Accordingly, although the refractions in his table represent Dr. BRADLEY's observations with great exactness as far as 86° from the zenith; yet, at lower altitudes, they diverge greatly from the truth; and the horizontal refraction comes out very nearly the same as in an atmosphere of uniform temperature. In this last hypothesis the refractions agree with nature as far as between 70° and 80° from the zenith; and, by means of the arbitrary quantity ε, they are bent to a conformity with observation a few degrees farther.

The preceding remarks have been made with the view of showing how it happens that the refractions in M. BESSEL's table agree with observation to a certain extent, and afterwards differ so widely from the true quantities. In comparing the two tables, we must attend to the points in which they are different from one another. In the table of M. BESSEL the constant of refraction is a little less than in the new table: the mean temperature is $48\frac{3}{4}$° of FAHRENHEIT in the former, and 50° in the latter; and the standard barometers are 29.6 inches and 30 inches. Now, supposing the two tables to represent the true mean refractions equally well, the differences we have mentioned will hardly have any other effect than to introduce a constant factor, by means of which the one table would be reduced to the other. The logarithms of the numbers in the two tables ought, therefore, to have constantly

the same difference; and how far this is actually the case, will appear by the inspection of the following table.

Distance from zenith.	Log. γ.		Difference.
	N. Table.	BESSEL.	
° 45	1.76612	1.75961	0.00651
55	1.92039	1.91385	0.00654
65	2.09568	2.08910	0.00658
75	2.33184	2.32510	0.00674
80	2.50541	2.49849	0.00692
81	2.54874	2.54175	0.00699
82	2.59624	2.58923	0.00701
83	2.64875	2.64174	0.00701
84	2.70740	2.70042	0.00698
85	2.77367	2.76683	0.00684
86	2.84951	2.84321	0.00630
87	2.93754	2.93246	0.00508
88	3.04122	3.03903	0.00219

As far therefore as 86° from the zenith, it appears that, in a practical point of view at least, the law of the refractions is the same in both tables; and the real difference between them is no more than a small variation in the constant of refraction. But, from 86° or 87° to the horizon, the two tables diverge so much from one another, that no comparison can be instituted between them.

The first instance of a rule for correcting the mean refractions different from the common one, which supposes that the variations are proportional to the changes in the density of the air, occurs in a formula of the eminent astronomer, T. MAYER, of Gottingen. The rule is given in the author's lunar tables without the demonstration; and it has been very generally misunderstood and decried;* so very uncertain is

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* See the Article Refraction in the *Tables Astronomiques*.

even an improvement in the abstruser parts of science. Doubts are entertained whether the rule was found by theory, or constructed in conformity to actual observation. The latter supposition cannot but seem very improbable when we attend to the formula; which is, besides, deduced very readily from the method of investigation pursued in this paper. M. Bessel is the first astronomer who has accurately computed all the variations of the refractions produced by the changes of temperature and pressure; and we shall next compare the new table in this respect with the result of his theory. Now, in his table, at the zenith distances 83°, 85°, 88°, the total corrections for the temperature τ are respectively, $-0''.9821(\tau-50), -1''.3678(\tau-50), 2''.9944(\tau-50)$: but each of these quantities involves the usual correction proportional to the change of the air's density, equal to $-\frac{\delta\theta}{480}\times(\tau-50)$; and, when this part is separated, they will stand as under;

$$\begin{aligned} \text{at } 83^{\circ}, &\ -0''.9131(\tau-50)-0''.0690(\tau-50),\\ 85, &\ -1.2178(\tau-50)-0.1500(\tau-50),\\ 88, &\ -2.2792(\tau-50)-0.7150(\tau-50),\end{aligned}$$

the latter parts being equal to $\frac{d\delta\theta}{d\tau}\times(\tau-50)$ in the notation of this paper. In the new Table the values of $\frac{d\delta\theta}{d\tau}\times(\tau-50)$ are respectively, $-0.074(\tau-50)$, $-0.159(\tau-50)$, $-0.722(\tau-50)$; differing insensibly from the calculations of M. Bessel.

To complete this examination of the new Table, we shall add some particular instances. We begin with the two examples at p. 159 of the *Connaissance des Tems.*

EXAMPLE I.

$\theta = 86^\circ\ 14'\ 42''$

Therm. 8°.75 Cent. = 47° ¾ FAHR.

Barom. $0^{m}.741$ = 29.17 In.

86° 10′	2.86325
4 42″	662
	2.86987
Therm.	0.00204
Barom. 9.98781 + 9	9.98790
Log. r - -	2.85981
$r = 724.1 =$	12′ 4″.1
$-0.28 \times -2\frac{1}{4}$	+ 0.6
$+0.8 \times -0.45$	− 0.4
	12 4.3
By observation	12 4.2
Error	+ 0.1
Error of French T.	2.2

EXAMPLE II.

$\theta = 86^\circ\ 15'\ 20''$

Therm. $8^\circ\frac{1}{8}$ = 46°.9 FAHR.

Barom. $0^{m}.766$ = 30.16 In.

86° 10′	2.86325
5 20″	752
	2.87077
Therm. - -	0.00281
Barom. 0.00232 + 13	0.00245
Log. r - -	2.87603
$r = 751.7 =$	12′ 31″.7
$-0.28 \times -3 =$	+ 0.8
$-0.16 \times -0.45 =$	+ 0.1
	12 32.6
By observation	12 32.5
Error	+ 0.1
Error of French T.	2.9

The next instance is more to the purpose, being the mean of 42 sub-polar observations of α Lyræ by Dr. BRINKLEY.*

$\theta = 87^\circ\ 42'\ 10''$

Therm. 35°

Barom. 29.5

87° 40′	3.00466
2 10″	390
	3.00856
Therm. - -	0.01379
Barom. 9.99270 + 64	9.99334
Log. r - -	0.01569
$r = 1036.8 =$	17′ 16″.8
-0.6×-15	+ 9 .0
-1×0.5	− 0 .5
	17 25.3
By observation - - -	17 26.5
Error - - - - - -	1.2
Error of French Tables	5.5

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* See his Paper on the Refractions, Irish Transact. 1815.

The error is here very small, as it ought to be in a mean of so many good observations. Half a degree subtracted from the thermometer would bring out an exact result; and some small difference may be fairly ascribed to the uncertainty arising from the different temperatures of the interior and exterior thermometers.

But it will be satisfactory to exhibit the errors of every particular observation. In the Irish Transactions for 1815, Dr. BRINKLEY has given 44 sub-polar observations of α Lyræ with the observed refractions; and these are contained in the following Table, extracted from the *Connaissance des Tems* for 1819, p. 418, the column of the errors of the new Table being added.

Date.	Barometer.	Thermometer.		Observed zenith distance.	Observed Refractions.	Corrections for F. Table.	Corrections of N. Table.
		In.	Out.				
				° ′ ″	′ ″		
1809, Jan. 22	29.25	25		87 42 1.6	17 57.4	+ 23.7	+ 12.0
Feb. 18	30.01	43½		40.7	24.8	+ 3.4	+ 3.8
20	29 78	43½		40.6	24.2	+ 10.7	+ 11.1
March 5	30.09	42½		33.0	34.7	+ 8.7	+ 8.6
12	30.05	44		22.1	46.2	+ 26.0	+ 26.5
1810, Feb. 13	29.94	34	30	57.0	3.1	— 3.5	— 8.1
19	30.02	32	29.5	5.9	55.6	+ 10.2	+ 3.0
March 17	29.62	36	33	31.0	33.4	+ 9.4	+ 5.5
1811, Jan. 18	29.90	33½	32	12.2	38.1	— 0.2	— 6.2
23	30.27	35	32½	41 55.1	56.6	+ 9.1	+ 4 6
28	29.35	27½	21½	58.5	54.6	+ 22.7	+ 13.3
Feb. 3	29 44	31½	30	42 34.3	20.4	— 7 7	— 14.8
7	29.24	39	38	52.5	3.2	— 2.8	— 4.2
8	29.28	39	35	51.2	4.7	— 2.3	— 4.0
12	29.03	38	34	58.4	16 58.4	— 2.6	— 5.8
13	28.91	35	33	43 3.3	53.7	— 10.2	— 14.0
Dec. 28	29.39	30½	25½	42 3.0	17 38.7	+ 12.2	+ 4.6
1812, Jan. 2	29.07	31½	30	22.0	21.2	+ 6.9	+ 0.3
3	28.95	29½	26½	34.0	9 5	— 5.8	— 13.7
4	29.11	27½	23½	41 56.2	47.6	+ 24.6	+ 15.4
7	29.93	32	31	42 2.1	42.6	+ 0.6	— 6.6
21	29.64	34	28½	1.2	47.9	+ 20.7	+ 15.3
30	29.18	39	35	36.4	19.2	+ 17.2	+ 25.2
Feb. 7	29.42	38	33	27.2	26.4	+ 13.9	+ 11.6
Dec. 22	29.66	33	26½	41 48.0	50.7	+ 21.1	+ 15.4
1813, Jan. 1	29 64	36	31	42 9.1	32.7	+ 9.4	+ 5.4
3	29.90	42½	40	23.0	19.5	+ 0.7	+ 0.8
11	29.52	36	31½	11.8	33.2	+ 14.2	+ 10.0
19	30.04	36	32	41 58.2	49.2	+ 12.0	+ 8.2
26	30.16	33	28	46.2	18 3.2	+ 16.1	+ 9.9
Feb. 6	29.40	39	38	42 46.8	17 5.6	— 5.1	— 7.1
15	28.50	40	38	43 24.8	16 29.6	— 10.0	— 11.1
18	29.26	39	37½	0.0	55.0	— 12.0	— 13.6
22	29.24	42	36½	42 52.3	17 3.3	+ 4.0	+ 4.1
Dec. 26	30.19	35½	31½	41 55.8	43.6	+ 0.1	— 4.7
27	30.01	36½	34	42 21.2	18.5	— 17.3	— 21.6
31	29.88	35½	33½	1.0	40.0	+ 7.5	+ 3.1
1814, Jan. 1	29.69	35	32½	21.2	20.1	— 8.4	— 12.8
4	29.11	26½	23	41 59.6	42.7	+ 17.4	+ 6.9
22	29.88	21	17	25.7	18 22.2	+ 18.3	+ 4.0
26	28.95	33	$32\frac{1}{1}$	42 56.2	16 52.8	— 16.5	— 21.6
27	28.78	32½	30½	49.8	59 4	— 4.2	— 9.9
29	28.63	31½	29	51.5	58.4	— 2.1	— 8.4
Feb. 13	29.67	41½	39	47.1	17 6.3	— 8.4	— 8.9

The errors of the French Table are + 340.8 — 119.1; amounting to 459.9 when the signs are neglected; and giving a mean error equal to $\frac{340.8 - 119.1}{44}$, or 5″.

In the new Table we have + 218.6 — 196.3; the total sum being 414.9, and the mean error $\frac{218.6 - 196.3}{44}$, or half a second.

In both views the advantage is in favour of the new Table.

The inspection of the foregoing Table will show how fruitless it would be to expect a near agreement in every single instance between observation and any table of refractions whatever. Thus, the zenith-distance is less, and the barometer and thermometer are both nearer the standard mean quantities on the 12th of March, than on the 5th of the same month, 1809; on all these accounts, the refraction should be less on the former day than on the latter; whereas, according to observation, it is greater by 11″.5. There is, therefore, no sure test of the accuracy of a Table of Refractions except the smallness of the mean error in a series of observations made at different times.

I shall now subjoin and compare with the new Table, observations of a number of stars at low altitudes, for which I am indebted to the liberality of Stephen Groombridge, Esq. F. R. S. The reductions necessary for finding the true refractions were made by that astronomer; and the practice of estimating the temperature of the air by the exterior thermometer, which he recommends as answering best with his method of observing, is followed in calculating from the

Tables. I have not thought it necessary to insert all the observations communicated by Mr. Groombridge; but such only, where the altitudes are less than five degrees.

The correction for temperature is variously computed by astronomers. Some use the interior, and others the exterior thermometer; and some prefer taking a mean between the two. But it may be affirmed with some degree of certainty, that the practice of computing by the exterior thermometer can be perfectly correct only when the temperature is the same within and without the Observatory. If we suppose that this is the case at first, and that afterwards the air within the Observatory is heated above, or cooled below, the external temperature; the consequence must certainly be, that the apparent place of a star will undergo some alteration. On the other hand, if the heat be equally distributed within the Observatory, and remain constant, while the temperature on the outside varies; it is not clear whether any change at all would be observed in the place of a star, more especially if the change of temperature were small.* But this is a point that can be determined only by careful experiments; and, until some light be thrown upon it, no great improvement can be expected in our knowledge of the astronomical refraction.†

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* See Dr. Brinkley's Paper, Philosophical Transactions, 1821, p. 335.

† N. B. In calculating the refractions, the temperature of the mercury in the barometer is estimated by the interior, that of the air by the exterior, thermometer.

Stars.	No. of Obs.	Mean Apparent Altitude.	Mean Observed Refractions.	Mean Bar.	Mean Ther. In.	Mean Ther. Out.	Corrections of French Table.	Corrections of New Table.
		° ′ ″	′ ″		°	°	″	″
11 Lacertæ.	16	85 4 10.5	10 10.5	29.91	47.5	38.3	—5.3	— 5.1
κ Androm.	12	85 4 37.8	10 3.7	29.72	51	43.5	—2.2	— 1.1
ξ Cygni.	15	85 10 51.3	10 23.3	29.92	46.6	38.5	—4.2	— 3.8
μ Ursæ Maj.	10	85 53 57.3	11 55.8	29.83	41	32.8	—3.7	— 3.5
ι Androm.	8	86 6 22.2	12 10.4	29.73	49.4	39.8	—3.3	— 2.6
γ Androm.	10	86 53 9.1	13 46.7	29.75	60.3	54.3	—7.0	— 1.2
ο Androm.	12	86 58 29.9	14 41.5	29.93	48.6	38.9	—2.9	— 2.7
β Bootis.	5	87 8 27.5	15 21.8	29.70	38.4	28.7	—9.7	—14.2
η Aurigæ.	9	87 18 57.8	15 14.2	29.85	60.2	56.3	—2.9	+ 5.1
ζ Aurigæ.	13	87 29 7.6	15 44.9	29.78	62.4	56.6	—6.4	+ 2.3
β Persei.	17	87 59 51.9	18 7.1	29.89	59.7	52.5	—6.6	+ 0.4
γ Cygni.	25	88 29 50.5	21 37.6	30.05	46.5	38.1	—0.9	— 7.2
ε Persei.	16	88 41 17.4	22 23.0	30.01	58.3	49.3	+6.9	+10.8

If we reject the observations of β Bootis and ε Persei, the errors of the French Table are all negative; and, in the New Table, the negative amount to more than triple the positive errors. Two different reasons may be assigned for the preponderance of the negative errors: it may be alledged that the refractions in the Tables are too great; or it may be said that, by using the exterior thermometer, the calculated refractions are increased more than in proportion to the real temperature of the air. The latter of these reasons is quite sufficient to account for the discordance; and it will receive additional force, if we attend to the great differences between the exterior and interior thermometers. In this case we cannot, therefore, draw any conclusion with the same confidence as in the preceding observations of Dr. Brinkley; but we may safely affirm, that the errors of the Table are not greater than the uncertainty of estimating the temperature of the air by the exterior thermometer.

TABLE I.

Mean Refractions for the Temperature of 50° *of* FAHRENHEIT, *and the Barometric Pressure* 30 *inches.*

Distance from Zenith.	δθ	Log. δθ	Difference.	Distance from Zenith.	δθ	Log. δθ	Difference.
°	' ''			°	''		
0	0 0			30	33.72	1.5279	173
1	1.02	0.0085	3012	31	35.09	1.5452	170
2	2.04	0.3097	1763	32	36.49	1.5622	168
3	3.06	0.4860	1252	33	37.93	1.5790	164
4	4.08	0.6112	974	34	39.39	1.5954	162
5	5.11	0.7086	796	35	40.89	1.6116	160
6	6.14	0.7882	675	36	42.42	1.6276	159
7	7.17	0.8557	587	37	44.00	1.6435	156
8	8.21	0.9144	519	38	45.61	1.6591	155
9	9.25	0.9663	466	39	47.27	1.6746	155
10	10.30	1.0129	424	40	48.99	1.6901	154
11	11.35	1.0553	388	41	50.75	1.7055	152
12	12.42	1.0941	359	42	52.57	1.7207	151
13	13.49	1,1300	334	43	54.43	1.7358	152
14	14.56	1.1634	313	44	56.35	1.7510	151
15	15.66	1.1947	294	45	58.36	1.76611	1512
16	16.75	1.2241	278	46	1 0.43	1.78123	1514
17	17.86	1.2519	265	47	2.57	1.79637	1518
18	18.98	1.2784	252	48	4.80	1.81155	1523
19	20.11	1.3036	241	49	7.11	1.82678	1530
29	21.26	1.3277	230	50	9.52	1.84208	1539
21	22.42	1.3507	222	51	12.02	1.85747	1551
22	23.60	1.3729	215	52	14.64	1.87298	1565
23	24.80	1.3944	207	53	17.38	1.88863	1577
24	26.01	1.4151	201	54	20.24	1.90440	1596
25	27.24	1.4352	195	55	23.25	1.92036	1617
26	28.49	1.4547	189	56	26.41	1.93653	1638
27	29.76	1.4736	185	57	29.73	1.95291	1664
28	31.05	1.4921	181	58	33.23	1.96955	1691
29	32.38	1.5102	177	59	36.93	1.98646	1722
30	33.72	1.5279		60	40.85	2.00368	

TABLE I. *continued.*

Distance from Zenith.	δθ	Log. δθ	Difference.	$\frac{d\,\delta\theta}{d\tau}$
° ′	′ ″			
60	1 40.85	2.00368	1756	
61	45.01	2.02124	1794	
62	49.44	2.03918	1836	
63	54.17	2.05754	1881	
64	59.22	2.07635	1932	
65	2 4.65	2.09567	1988	
66	10.48	2.11555	2048	
67	16.78	2.13603	2116	
68	23.61	2.15719	2191	
69	31.04	2.17910	2275	
70 00	39.16	2.20185	388	
10	40.59	2.20573	390	
20	42.04	2.20963	393	
30	43.52	2.21356	396	
40	45.02	2.21752	398	
50	46.53	2.22150	402	
71 00	48.08	2.22552	404	
10	49.65	2.22956	407	
20	51.25	2.23363	410	
30	52.87	2.23773	413	
40	54.53	2.24186	417	
50	56.21	2.24603	419	
72 00	57.92	2.25022	423	
10	59.66	2.25445	425	
20	1.43	2.25870	429	
30	3.23	2.26299	433	
40	5.06	2.26732	436	
50	6.93	2.27168	440	
73 00	8.83	2.27608	443	
10	10.77	2.28051	447	
20	12.74	2.28498	450	
30	14.75	2.28948	454	
40	16.80	2.29402	458	
50	18.88	2.29860	462	
74 00	21.01	2.30322		
° ′	′ ″			
74 00	3 20.01	2.30322	467	
10	23.18	2.30789	470	
20	25.39	2.31259	475	
30	27.66	2.31734	479	
40	29.95	2.32213	483	
50	32.30	2.32696	488	
75 00	34.70	2.33184	493	0.009
10	37.16	2 33677	497	
20	39.65	2.34174	502	
30	42.21	2.34676	507	
40	44.82	2.35183	512	
50	47.48	2.35695	517	
76 00	50.21	2.36212	523	0.012
10	53.00	2.36735	528	
20	55.85	2.37263	533	
30	58.76	2.37796	538	
40	4 1.74	2.38334	545	
50	4.79	2.38879	551	
77 00	7.91	2.39430	557	0.015
10	11.11	2.39987	563	
20	14.39	2.40550	569	
30	17.74	2.41119	576	
40	21.19	2.41695	583	
50	24.72	2.42278	589	
78 00	28.33	2.42867	596	0.018
10	32.04	2.43463	603	
20	35.84	2.44066	611	
30	39.75	2.44677	618	
40	43.76	2.45295	626	
50	47.88	2.45921	635	
79 00	52.12	2.46556	642	0.023
10	56.47	2.47198	650	
20	5 0.94	2.47848	659	0.026
30	5.54	2.48507	669	
40	10.28	2.49176	677	
50	15.16	2.49853	688	
80 00	20.19	2.50541		0.030

TABLE I. *continued.*

Distance from Zenith.	$\delta\theta$	Log. $\delta\theta$	Diff.	$\frac{d\delta\theta}{d\tau}$	$\frac{d\delta\theta}{dp}$	Distance from Zenith.	$\delta\theta$	Log. $\delta\theta$	Diff.	$\frac{d\delta\theta}{d\tau}$	$\frac{d\delta\theta}{dp}$
° ′	′ ″					° ′	′ ″				
80 00	5 20.19	2.50541	696	0.030	0.04	85 00	9 53.84	2.77367	1191	0.159	0.25
10	25.36	2.51237	707	0.031		10	10 10.35	2.78558	1219	0.171	0.26
20	30.70	2.51944	716	0.034		20	27.73	2.79777	1248	0.184	0.28
30	36.20	2.52660	727	0.034		30	46.03	2.81025	1277	0.198	0.31
40	41.88	2.53387	738	0.036		40	11 5.30	2.82302	1309	0.213	0.33
50	47.74	2.54125	749	0.038		50	25.66	2.83611	1340	0.229	0.36
81 00	53.79	2.54874	759	0.040	0.05	86 00	47.15	2.84951	1374	0.248	0.39
10	6 0.04	2.55635	772	0.042		10	12 9.88	2.86325	1410	0.269	0.43
20	6.50	2.56407	785	0.044		20	33.97	2.87735	1447	0.292	0.47
30	13.18	2.57192	797	0.046	0.07	30	59.51	2.89182	1484	0.317	0.51
40	20.09	2.57989	811	0.049		40	13 26.61	2.90666	1523	0.345	0.56
50	27.26	2.58800	824	0.051		50	55.40	2.92189	1565	0.376	0.62
82 00	34.68	2.59624	838	0.053	0.08	87 00	14 26.04	2.93754	1608	0.410	0.68
10	42.37	2.60462	851	0.057		10	58.71	2.95362	1654	0.448	0.75
20	50.33	2.61313	866	0.060		20	15 33.60	2.97016	1701	0.490	0.83
30	58.59	2.62179	883	0.063	0.10	30	16 10.89	2.98717	1749	0.538	0.91
40	7 7.19	2.63062	899	0.067		40	50.8	3.00466	1801	0.593	1.01
50	16.13	2.63961	914	0.071		50	17 33.6	3.02267	1855	0.654	1.13
83 00	25.40	2.64875	931	0.074	0.11	88 00	18 19.6	3.04122	1909	0.722	1.26
10	35.05	2.65806	949	0.079		10	19 9.0	3.06031	1967	0.799	1.41
20	45.10	2.66755	967	0.084		20	20 2.2	3.07998	2026	0.887	1.59
30	55.58	2.67722	986	0.089	0.13	30	59.6	3.10024	2089	0.987	1.79
40	8 6.50	2.68708	1006	0.095		40	22 1.7	3.12113	2155	1.101	2.02
50	17.90	2.69714	1026	0.101		50	23 8.9	3.14268	2221	1.231	2.29
84 00	29.80	2.70740	1047	0.107	0.16	89 00	24 21.8	3.16489	2290	1.380	2.61
10	42.24	2.71787	1069	0.114		10	25 40.9	3.18779	2361	1.551	2.98
20	55.25	2.72856	1092	0.122		20	27 7.1	3.21140	2434	1.749	3.41
30	9 8.88	2.73948	1115	0.130	0.20	30	28 40.8	3.23574	2509	1.977	3.93
40	23.16	2.75063	1139	0.139		40	30 23.2	3.26083	2584	2.241	4.54
50	38.12	2.76202	1165	0.149		50	32 15.0	3.28667	2667	2.549	5.26
85 00	53.84	2.77367		0.159	0.25	90 00	34 17.5	3.31334		2.909	6.12

TABLE II.—*Thermometer.*

°		Diff.	°		Diff.
50	0.00000		50	0.00000	
49	0.00090	91	51	9.99910	
48	0.00181		52	9.99820	
47	0.00272		53	9.99730	
46	0.00363	92	54	9.99640	
45	0.00455		55	9.99550	
44	0.00546		56	9.99460	90
43	0.00638		57	9.99371	
42	0.00730		58	9.99282	
41	0.00822		59	9.99193	
40	0.00914		60	9.99104	
39	0.01006	93	61	9.99016	89
38	0.01099		62	9.98927	
37	0.01192		63	9.98839	
36	0.01285	94	64	9.98751	
35	0.01379		65	9.98663	
34	0.01472		66	9.98575	88
33	0.01566		67	9.98488	
32	0.01660		68	9.98401	
31	0.01754		69	9.98314	
30	0.01848		70	9.98227	
29	0.01942	95	71	9.98140	87
28	0.02037		72	9.98054	
27	0.02132		73	9.97967	
26	0.02227		74	9.97881	
25	0.02323	96	75	9.97795	
24	0.02418		76	9.97709	
23	0.02513		77	9.97623	
22	0.02609		78	9.97537	86
21	0.02706	97	79	9.97452	
20	0.02803		80	9.97367	
19	0.02900		81	9.97282	
18	0.02997		82	9.97197	
17	0.03094		83	9.97112	
16	0.03191		84	9.97027	85
15	0.03288	98	85	9.96943	
14	0.03386		86	9.96859	
13	0.03484		87	9.96775	
12	0.03582		88	9.96691	
11	0.03680	99	89	9.96607	84
10	0.03779		90	9.96524	

TABLE III.—*Barometer.*

°		Diff.	°		Diff.
30	0.00000		30	0.00000	
30.1	0.00145		29.9	9.99855	145
30.2	0.00289	144	29.8	9.99709	
30.3	0.00432		29.7	9.99563	
30.4	0.00575		29.6	9.99417	146
30.5	0.00718	143	29.5	9.99270	
30.6	0.00860		29.4	9.99123	147
30.7	0.01002	142	29.3	9.98975	148
30.8	0.01143		29.2	9.98826	
30.9	0.01284	141	29.1	9.98677	
31.0	0.01424	140	29.0	9.98528	149
			28.9	9.98378	
			28.8	9.98227	
			28.7	9.98076	151
			28.6	9.97924	
			28.5	9.97772	
			28.4	9.97620	152
			28.3	9.97466	
			28.2	9.97313	
			28.1	9.97158	
			28.0	9.97004	154
			27.9	9.96848	
			27.8	9.96692	156
			27.7	9.96536	157
			27.6	9.96379	
			27.5	9.96221	158

TABLE IV.

°	+		°	—
50	0.00000		50	0.00000
40	0.00043		60	0.00043
30	0.00087		70	0.00087
20	0.00130		80	0.00130
10	0.00173		90	0.00173

This Table of refractions has been constructed merely with the view of comparing the theory contained in the Paper with observation. The elements are precisely the same as those of the French Table in all other respects excepting the quantity f, which is assumed equal to $\frac{1}{4}$, from the exact manner in which this value seems to represent terrestrial observations. But it would be more satisfactory to determine the same quantity by the comparison of many observed refractions at low altitudes, between the distances of 85° and 88° from the zenith; and by this means a Table might be constructed that would be deserving of greater confidence.

J. IVORY.

XXIX. *Observations on Air found in the Pleura, in a case of Pneumato-thorax; with experiments on the absorption of different kinds of air introduced into the pleura.* *By* John Davy, *M. D. F. R. S.*

Read June 6th, 1823.

I trust that the following case of pneumato-thorax, with the experiments made to illustrate it, will not prove undeserving of the attention of the Royal Society, which, from its commencement, has warmly encouraged physiological inquiries, and every investigation directed to the improvement of medical science.

Abraham Iredill, of the 7th regiment of Foot, aged 30, was admitted into the General Military Hospital at Fort Pitt, Chatham, on the 15th of January last, labouring under phthisis pulmonalis, and invalided on account of it.

His disease exhibited some peculiarities, the cause of which was not discovered during life, the chest not having been minutely examined by exposure and percussion, owing to the severity of cold at the time and the hopeless state of the patient, evidently on the brink of the grave. He expired on the 11th of February, and his body was inspected the day following, fourteen hours after death.

The right side of the chest exhibited a great degree of fulness, and it emitted, when struck, a hollow sound. On carefully opening the abdomen, the diaphragm was found protruding into the right hypochondrium, exhibiting a surface convex, and almost conical instead of concave; and it was

tense and tympanitic. The right lobe of the liver was pressed into the epigastrium, and rested on a portion of the stomach and duodenum and a part of the transverse colon. Owing to the pressure of the liver, the pyloric portion of the stomach was removed from its natural situation to the left iliac region, where it rested on the upper part of the sigmoid flexure of the colon; and, owing to the same pressure, the small intestines generally were driven downwards, and more or less displaced.

The body was put into a bath, and a small opening was made, under water, with a scalpel, into that part of the right pleura, best adapted by situation to allow the escape of air. Air issued out in abundance: 212 cubic inches were collected in receivers, and about 13 cubic inches escaped, making altogether the enormous volume of 225 cubic inches. The air collected was set aside for examination, and the body being replaced on the table, a portion of the ribs was removed from the right side to admit of the examination of the chest, the water that had rushed in to supply the place of the air having been carefully taken out and preserved.

The inner surface of the right pleura was covered with a thin layer of coagulable lymph. The right lung was exceedingly compressed: it adhered closely to the upper part of the pericardium, and loosely to the posterior part of the chest (about the sixth and seventh ribs) by a few strong bands.

On inflating the lungs with a double bellows through an opening into the trachea, the right lung became much expanded, and air was found to pass freely from the lung into the pleura through an ulcerated opening in the upper part of the superior lobe.

The right lung was carefully dissected out. In the upper part of its superior lobe " a tubercular excavation," or vomica, was found, of the capacity of about four ounce measures, which communicated with the aspera arteria by a large bronchial tube, the ulcerated end of which terminated in the side of the excavation opposite to the openings by which the vomica communicated with the pleura.

On examining minutely the communication between the cavity of the chest and the lung, a kind of valvular structure was discovered, which would allow of air being pumped into the pleura in the act of inspiration, but not of its escape in expiration, owing to which, no doubt, the accumulation of air in question took place. Even at the risque of being tedious, I must attempt to convey some idea of this structure. Between the false membrane of the vomica and the pleura there was a small irregular sinus, not exceeding an inch in diameter, the sides of which though not adhering, of course were in contact, or very nearly so. This sinus was the channel of communication, and contained the valvular structure alluded to. It opened into the cavity of the chest by a hole in the pleura pulmonalis about the size of a crow-quill, and into the vomica by three smaller holes in the substance of the lung, not corresponding with the former, so that a probe could not be passed from one into the other in a straight line ; and, consequently, when the surfaces of the sinus were pressed together by the compression of the air in the pleura in the act of expiration, the communication through which the air entered was closed, and its exit prevented.

I shall now proceed with the description of the remaining morbid appearances. Besides the vomica described in the right

lung, this viscus contained small tubercles, few in number in the inferior lobe, but abundant in the superior lobe. The largest of them did not exceed in size a common pea, and the smallest were not larger than mustard seed. The smallest were translucent; the larger were of different degrees of opacity; all of them were solid, and none of them had suppurated. The left lung was free from adhesion; like the right, it contained numerous small tubercles, that had made very little progress. The bronchia, and the lower part of the trachea, were redder than natural. There were three ounces of serum in the pericardium, and a larger quantity of fluid than usual in the ventricles of the brain. No air was observable in the blood vessels or in the cellular membrane of any part of the body.

I shall now return to the contents of the right pleura. The water taken from the pleura (viz. that which entered when the air was discharged,) was turbid, as if from the admixture of pus. After resting 24 hours in tall glass jars, a white sediment formed, which, carefully separated by decantation, was about an ounce in quantity. It had the appearance of pus, and exhibited the properties of pus when examined by the most approved tests:—thus it became viscid with a solution of muriate of ammonia; it was soluble in sulphuric acid, and precipitable by dilution with water; and it produced coloured rings when placed between two surfaces of glass and held before a candle, according to the method recommended by Dr. YOUNG. The decanted fluid, examined by solution of corrosive sublimate and by evaporation, was found to contain serum; and, judging from the extract it afforded, it was about eleven ounces in quantity; half an ounce of the decanted water having yielded, when evaporated, 2.2 grains of dry residue.

The air collected from the pleura had not the least fœtor, nor indeed any smell. It extinguished flame, and was not inflammable. Examined by means of lime-water and phosphorus (which was sublimed in it without effect,) 100 parts of it were found to consist of 8 carbonic acid gas, and 92 azotic gas.

Whence this air was derived became a question for consideration. Reflecting on the communication, discovered by dissection, between the pleura and the atmosphere through the medium of the lung, it seemed almost demonstrated, that the air was atmospheric air altered.

The next question that presented itself was, how the alteration had taken place; what had become of the oxygene that had disappeared; whence the carbonic acid gas with which the azote was mixed?

To endeavour to learn how the oxygene had disappeared, the following experiment was instituted. The right pleura of a dog was inflated with atmospheric air by means of a double bellows, and the incision through which the air was introduced was closed by a suture. At the end of 48 hours the dog was killed. An hour after death the pleura was punctured under water, and about 8 cubic inches of air were collected, which, examined by means of lime-water and phosphorus, were found to contain slight traces of carbonic acid gas, and to consist of 93 parts azotic gas, and 7 oxygene gas. The wound in the pleura was closed by coagulable lymph, and the pleura was found free from inflammation.

The result then of this experiment seems to show, that the oxygene was absorbed in a greater proportion than the azote;

and thus tends to account for the accumulation of the latter gas in the preceding case.

It may be said, that the experiment does not warrant the inference that any azote was absorbed, and, consequently, that the expression "in a greater proportion," is incorrect. The absorption of this gas is probable, however, though not demonstrated in the present instance, as Sir ASTLEY COOPER has found that common air introduced into the cellular membrane, and into the cavity of the thorax and abdomen of dogs, is, after a certain time, entirely removed by absorption.*

Relative to the source of the carbonic acid gas, it is easy to conceive that it was formed, or emitted, in the air cells of the lungs, as in ordinary respiration; and that, mixing with the air inspired, it was received into the pleura. If thus derived, and not from the surface of the pleura by secretion, it seems to follow, that it is less readily absorbable by the pleura, than oxygene. To endeavour to decide this point, the following experiment was made.

About 30 cubic inches of air, consisting of 80 parts common air and 20 carbonic acid gas, were passed from a receiver into a bladder, furnished at one extremity with a stop-cock, and at the other with a small trochar; both air tight. A small incision having been made through the integuments of the right side of the chest of a dog, the trochar was passed through the intercostal muscles into the pleura. The stillette was immediately drawn from the cannula into the bladder, and the air of the bladder instantly rushed into the pleura, and, on expiration, was in part forced back into

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* Surgical and Physiological Essays by JOHN ABERNETHY, p. 55; London, 1793.

the bladder. The exact quantity of air retained was not determined; it must have exceeded at least ten cubic inches. As speedily as possible the cannula was withdrawn, and the external wound carefully closed by suture. The health of the dog was very little impaired by this operation. Two days after, when the animal appeared to be quite well, a similar experiment was made on the left side of the chest, and a mixture, consisting of 75 parts common air and 25 carbonic acid gas, was introduced into the pleura. This operation had very little more effect than the former. At the end of 24 hours the dog was killed, and immediately examined.

About 3 cubic inches of air only were procured from the left pleura, which were found to consist of

18.3 carbonic acid gas,
78.3 azotic gas,
3.4 oxygene gas:

whilst the air admitted consisted of

20.0 carbonic acid gas,
63.2 azotic gas,
16.8 oxygene gas:—

Thus apparently showing, that during a sojourn of three days in the pleura, the oxygene had been absorbed in a greater degree than the carbonic acid gas, and the latter in a greater degree than the azote. The result of the experiment on the left pleura was very similar; it afforded ten cubic inches of gas, consisting of 25 carbonic acid gas, 70.6 azotic gas, and 4.4 oxygene gas. The appearances on examining the wounds were satisfactory: the cavity of the chest was free from inflammation, the lungs uninjured, and the cicatrix in the pleura only just perceptible.

The results of these experiments seem to warrant the conclusion, that in the preceding case the carbonic acid gas found, was not derived from the surface of the pleura by secretion or exhalation, but from the respired air through the ulcerated opening. And with this remark I shall dismiss the case of Pneumato-thorax, the consideration of which, as a medical subject, would not be appropriate to this place.

The power exhibited by the pleura in the preceding instances of absorbing gases, and the manner in which it exercised that power, in a greater degree, on one air than on another, and that in no ratio to their solubility in water, appeared to me so interesting and novel, that I was induced to prosecute the subject a little farther. With the same apparatus, I made similar experiments on the admission of three other gases into the pleura of dogs, viz. hydrogene, nitrous oxide, and nitrous gas, the results of which I shall briefly describe.

About 20 cubic inches of a mixture, consisting of 57.5 parts carbonic acid gas, and 42.5 hydrogene were admitted into the left pleura of a dog, in the manner, and with the precaution already noticed. The health of the animal was not apparently affected. At the end of two days, about 30 cubic inches of a mixture, consisting of 44.5 azote, and 55.5 nitrous gas, were passed into the right pleura. Immediately the dog's breathing became quick and short, but not laborious. It refused to eat, and expired in the evening, at the end of five hours from the time that the air was introduced. The next morning the body was examined. About six cubic inches of air were collected from the left pleura, consisting, apparently, of 12 carbonic acid gas and 88 azote. After the

removal of the carbonic acid gas by lime water, the residual gas extinguished flame, and was not itself in the least inflammable; whence the inference that it was azote, or at least principally azote, as the presence of a small quantity of hydrogene might be concealed, and escape detection. From the right pleura, about five cubic inches of air were procured, which consisted of 6.9 nitrous gas, or air absorbable by a solution of green sulphate of iron, and of 93.1 azote. On opening the chest, the wounds in the pleura were found closed; the pleuræ were of natural appearance; the substance of the left lung was redder than usual, and that of the right was dark red, and it contained a good deal of blood and serum; the bronchia did not exhibit decided marks of inflammation; the right auricle and ventricle and the venæ cavæ were distended with grumous blood, and the left auricle and ventricle and aorta contained a good deal of liquid blood, which, as well as that of the venous system, had lost its peculiar tint, and had acquired a chocolate hue.

The obvious results of these two experiments on the same dog, are, 1st. the absorption of the greater part of the carbonic acid gas, and the whole of the hydrogene introduced into the pleura, and the appearance, *de novo*, of a considerable quantity of azote:—2dly, the death of the animal in the space of five hours from the time of admission of the nitrous gas and azote into the opposite pleura, the absorption of the greater part of the former gas without inflammation of the membrane with which it was immediately in contact, and the production of a peculiar change in the blood.

Results so singular as these required to be narrowly scrutinized. I have twice repeated the experiment on the admis-

sion of hydrogene into the pleura of dogs, and in each instance after death I found that the hydrogene had disappeared, and that its place was supplied by a small quantity of azote.

Did the azote found in these instances exist in the pleura previous to the experiment?

A remark of Dr. LAENNEC, would seem to countenance this notion. He says, " M. RIBES assures me, that he has found in opening the serous cavities of dogs a small quantity of air constantly to escape."* On the contrary, in opposition to this, are the experiments of HALLER and other accurate observers, recorded in the controversy which HAMBERGERUS gave rise to, by reviving and maintaining the opinion of GALEN, that air is contained between the lungs and the pleura.†

In doubt between these contending authorities, with the desire of satisfying myself on the point, I have made some experiments on dogs, the results of which convince me, that in a healthy state, no air is contained in the pleura of this animal. When I opened, under water, the chests of dogs killed by drowning, not the smallest globule of air escaped; but, when the right side of the chest was opened in the atmosphere, an appearance presented itself, at first favourable to the idea of a little air being contained in the left pleura, for the mediastinum was pressed from the left side towards the right, (the body lying on the left side) evidently by air within the transparent membrane. This appearance on examination proved to be fallacious, for the air was found to

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* A Treatise on the Diseases of the Chest, &c. translated from the French of R. T. H. LAENNEC, M. D. by JOHN FORBES, M. D. p. 208.

† HALLER's Not. in Prælect. BOERH. DCVI. HALLER's Opuscula Anatomica, de resp. Gott. 1751, p. 91, and 345. MARHERR's Prælect. in BOERH. Inst. vol. iii. p. 391.

be not in the left pleura, but in a cavity of the mediastinum communicating with the right pleura, and containing a lobule of the right lung, *dextri pulmonis additamentum*, as HALLER calls it, who has noticed this structure in the mediastinum of the dog and many other animals, and pointed it out as one of the principal causes of the erroneous notion that he combated.*

Was the azote derived from the blood as an exhalation or secretion?

Facts might be advanced in favour of this idea. An exhalation, or disengagement of azote appears to have taken place in the experiment of Messrs. ALLEN and PEPYS, when oxygene nearly pure was respired.† In the inspection of dead bodies, air has frequently been found in the vessels and closed cavities, which is probably azote.‡ It has been asserted

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* HALLER's Opuscula Anatomica, p. 44. † Phil. Trans. 1809.

‡ Vide MORGAGNI De sed. et Causis Morb. Epist. v. and Transactions of a Society for the Improvement of Med. and Chir. Knowledge, vol. i. in which an interesting "case of Emphysema not proceeding from local injury," with some important observations relative to the secretion of air, is given by Dr. BAILLIE.

Notwithstanding the experiments detailed by Sir EVERARD HOME, in his Croonian Lecture, published in the Phil. Trans. for 1818, I am induced to believe that the gas in question is azote, rather than carbonic acid; because the alkali in the blood is not saturated with carbonic acid; because the serum of blood is capable of absorbing carbonic acid gas, rather more even than water, as I have ascertained by experiment; because, during the coagulation of blood spontaneously, and the coagulation of serum by heat, I have never observed carbonic acid gas to be disengaged, when the experiments were properly made in vessels to which air could not have access, as in tubes completely filled with blood or serum, and inverted in blood or mercury; and lastly, because I have not been able to procure carbonic acid gas from blood just drawn from the vessels, and still warm, when placed under a receiver, and completely exhausted of air. I may here remark, that I have made two experiments on blood in vacuo, and in both with the same negative results. In one instance the arterial blood of an ox was employed, and in the other the blood of a man in health. In the former eight ounces were used, in the latter one ounce. In both instances

lately, that air thus found is, in every instance, the consequence of putrefaction. But surely the accurate MORGAGNI was not so egregiously deceived. Many times I have noticed air in the vessels of the pia mater, in bodies only a few hours dead, and very lately I detected some in the internal jugular veins of a body that had been dead only eighteen hours, and free from every mark of incipient putrefaction: and I lay the more stress on this observation, because it was very carefully made before any large vessel was divided through which air could gain admission. Farther, air seems to pass pretty readily (probably through the exhalants) from the air cells of the lungs into the pleura. Is not this proved by an experiment of HALES ?* And an experiment which I have made, and which I may briefly notice, seems to afford some proof of it. Immediately after death, before the muscles had lost their irritability, I inflated the lungs of a dog under water by means of a double bellows, through the trachea. Air in exceedingly minute bubbles escaped from the surface of the pleura covering one of the inferior lobes; and on making gentle pressure with the fingers on any part of the inflated viscus, the same appearance presented itself.

These circumstances, which I have ventured to bring forward as somewhat favourable to the idea of the secretion or exhalation of azote, are still far from conclusive. After having given the subject all the attention in my power, I do not venture to draw a positive conclusion. I have thought it

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the blood remained perfectly tranquil, when the vacuum was as complete as could be made with a good air pump, and of course did not exhibit the slightest traces of the disengagement of any air.

* Vide Stat. Essays i. 252.

right to state what I have observed relative to a topic so interesting and obscure; and to notice such facts as seemed to bear more immediately on the question, in hope of exciting farther inquiry, by which alone the true source of the azote, apparently evolved in the preceding instance, can be ascertained.

The effect of nitrous gas introduced into the pleura now requires consideration. I have made several trials farther with this gas. When admitted nearly pure into the pleura it produced very serious symptoms, but did not prove fatal, provided the lung on the opposite side was free to act. The distressing symptoms usually subsided in about twelve hours; and then, on killing the animal, the greater part of the nitrous gas was found to be absorbed; the pleura was free from inflammation, the substance of the lungs very slightly inflamed, and the blood exhibited a brownish hue. From these circumstances it may be conjectured, that nitrous gas produces its deleterious effects after it has been absorbed, either by acting on the blood immediately, or on the air cells of the lungs and the blood conjointly, when converted into nitrous acid in the course of the pulmonary circulation.

On the admission of nitrous oxide into the pleura, I have made one experiment only. About 30 cubic inches of this gas, contaminated with 25 per cent. common air, were passed into the pleura of a dog. The animal exhibited no uneasy feeling, and immediately after appeared to be rather exhilirated. It continued apparently in good health for 24 hours, when it was killed. Five cubic inches of air were procured from the pleura, which consisted of 10 per cent. oxygene and 90 azote, being quite deprived of nitrous oxide. The pleura and lung

exhibited no unusual appearances that could be referred to the gas absorbed.

Mr. Abernethy, in his ingenious Essay on the Functions of the Skin, has proved that that texture is possessed of a power of absorbing and exhaling certain gases, which it exercises according to laws peculiar to the animal economy.* The preceding experiments seem to show that the pleura is possessed of a similar power in respect to absorption, and that in exerting this power, like the skin, it prefers one gas to another. Whether the analogy will hold good as regards exhalation also, must be decided by future inquiry

Fort Pitt, Chatham, April 12, 1823.

APPENDIX,

CONTAINING

An account of a case of Pneumato-thorax, in which the operation of tapping the chest was performed; with some additional observations on air found within the body; and on the power of mucous membranes to absorb air.

In the preceding pages I have given some particulars of a case of tubercular consumption, which proved rapidly fatal in consequence of the supervention of Pneumato-thorax; I have now the honour of communicating another case, in which the existence of air in the cavity of the chest was detected during life, and the patient was relieved from very distressing and

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* Surgical and Physiological Essays, Part II. by John Abernethy.

alarming symptoms, by perforating the chest, and allowing the accumulated air to escape.

PATRICK CALNON, of the 50th regiment of Foot, was admitted into the medical division of the General Military Hospital; at Fort Pitt, on the 9th of May last, immediately on his return from Jamaica, from whence he was sent home invalided on account of hæmoptysis, produced by a severe fall on the left side of the chest, 18 months ago, previous to which accident he had enjoyed uninterrupted good health.

Till the 13th of May his complaint exhibited nothing peculiar. Early on the morning of that day, after a violent fit of coughing, the symptoms of pneumato-thorax began to appear, and they continued to increase till the 21st. The most prominent symptoms were, a feeling of extreme tightness about the chest and abdomen; rapid and difficult respiration, between 30 and 40 in a minute; great anxiety of countenance and agitation of mind, accompanied with a small pulse of 130; cold sweats frequently breaking out on the face and neck; and a considerable prostration of strength. On examining the chest, the left side was found more protuberant, and in all its dimensions larger than the right; it was tense, and on percussion sounded remarkably hollow and tympanitic, giving the idea of its being distended with air; and the heart was found beating on the right side under the mamilla.

In consultation with Dr. SKEY, Physician to the Forces, and Mr. SCHETKEY, Surgeon to the Forces, the operation of tapping the chest, which I recommended, was approved of, and, with the consent of the patient, immediately performed.

With a small trochar, attached to a flaccid bladder, I carefully perforated the left side of the chest, between the 8th

and 9th ribs, having previously divided the integuments and the intercostal muscles with a scalpel. On withdrawing the stillette a little air rushed out, and was collected in the bladder, but not in the quantity that I expected; it did not exceed five cubic inches; and on examination it was found to consist of azote, and a little carbonic acid.

Conceiving that the operation had failed in consequence of adhesions in the part of the pleura punctured, and, encouraged by the composition of the air collected, and the slight relief which the patient experienced, a repetition of the operation was decided on, and performed the next day.

The chest was perforated just below the left papilla. Now, on withdrawing the stillette into the bladder, a large quantity of air rushed out and distended the bladder; and on separating the bladder from the cannula (by cutting it off, after having secured the air in the former by a tight ligature,) air from the chest continued for several seconds to rush out with violence, as if from a blow-pipe. When the rushing of air ceased, and it was found that air began to pass in on inspiration, the cannula was withdrawn, and the wound was closed by adhesive plaster.

The patient experienced sudden and great relief, exceeding his power to express. Since the operation he has continued to improve, and now, June 17, he is as well as, or better, than when first admitted into the hospital; his appetite is good, his cough little troublesome; he can lie on the left side, which he was unable to do for many months prior to the operation; both wounds are healed; and the left side of the chest is diminished considerably in volume, and is much less tense and tympanitic. Though the heart still beats

on the right side, and the fluctuation of a fluid in the left cavity of the chest is very distinct on any sudden motion of the body, I indulge the hope, if his lung be not tuberculated, that he will eventually recover completely.

The air collected in the bladder amounted to 25 cubic inches. Examined by means of lime-water and phosphorus, it was found to consist of 93 azotic gas and 7 carbonic acid gas: thus, in composition, proving almost exactly the same as the air found in the fatal case described in the preceding part of this Paper; and, as in that instance, it had not the least offensive smell.

Whether the origin of the air in this instance was the same as in the former, it is not easy at present to decide; most probably it was, considering the nature of the preceding disease, and the sudden supervention of the symptoms of pneumato-thorax after a violent fit of coughing; when it is likely the pleura was ruptured, and a kind of valvular communication established between its cavity and the aspera arteria, permitting air to enter, and preventing its return.

Relative to the secretion of air in the human body, and its effusion into closed cavities, I have ventured in this Paper to make some remarks, and to express an opinion in favour of such an occurrence. Very recently I have met with two fatal cases, in which shortly after death, and before there was the least indication of putrefaction having commenced, I detected air, apparently secreted and accumulated in sufficient quantity to admit of its being collected and examined. As the subject is quite novel, I trust a brief notice of these two instances may prove not unacceptable to the Royal Society.

On the 23rd of May, on examining the body of a soldier, aged 27, who had died of chronic dysentery, complicated with an ulcer of the larynx, the cellular membrane in both mediastina was found vesicular, and distended with air. The vesicles were burst under water, and a half cubic inch of air collected, which was found to consist of

7 oxygene,
4 carbonic acid gas,
89 azote.

The surrounding parts were carefully examined, particularly the trachea, lungs, and œsophagus; but no passage could be detected through which air could have entered the mediastina; nor could any air be forced into them by distending the lungs by means of a double bellows. Probably the oxygene found was extraneous, and was derived partly from common air adhering to the surface of the cellular membrane, and partly by penetrating through the delicate vesicles during the preparatory dissection, when they were exposed to the atmosphere for half an hour at least.

On the 2nd of June, on examining the body of a soldier, aged 36, who had died of tubercular consumption, I found air vesicles on the surface of the lungs, similar to those described by Dr. Baillie in his Morbid Anatomy,* and considered by him as formed by the secretion of air, and not by the extravasation of air under the pleura, agreeably to the opinion lately advanced by Dr. Laennec.† The air contained in the vesicles, in this instance, consisted of 5 parts azote and 1 part carbonic acid. The quantity of air collected

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* Fifth edit. p. 80.

† A Treatise on the Diseases of the Chest, &c. p. 89.

and examined did not exceed $\frac{1}{20}$ of a cubic inch, and I could not detect in it any traces of oxygene.

After the experiments detailed in this Paper were made, on the absorption of different kinds of air introduced into the pleura, it appeared probable, on reflection, that mucous membrane, like serous membrane and the skin, might possess the power of absorbing air. In relation to this view, I thought it worth while to examine the air contained in the antrum maxillare and in the frontal sinus. I chose for the experiment the head of the sheep, in which these cavities are large, the openings by which they communicate with the atmosphere small, and the membrane with which they are covered, an active secreting surface. I collected the air by perforating the cavities under water about 15 minutes after the death of the animal. In two different instances the results of the examination of the air were the following: the air from the antrum maxillare in one instance consisted of

4.3 carbonic acid gas,
13.0 oxygene,
82.7 azote;

from the frontal sinus of

13.5 oxygene,
86.5 azote,

without any carbonic acid gas, the absence of which may have been owing to the presence of a good deal of mucus in the cavity, by which it might have been absorbed. In another instance the air from the antrum maxillare consisted of

4.2 carbonic acid gas,
13.8 oxygene,
82.0 azote;

from the frontal sinus of

4.5 carbonic acid gas,
9.5 oxygene,
86.0 azote.

On the supposition that the air, previous to its entering these cavities, had undergone a partial change from respiration, the results described seem to indicate an absorption of oxygene.

Other facts might be adduced, which, like the preceding, though not conclusive, tend to support the idea, that mucous membranes are capable of absorbing air. Of this kind, I conceive, are the results of the experiments of Messrs. MAGENDIE and CHEVREUL, on the composition of the air contained in the human stomach and intestines;* and very recently, I have met with a fact, the bearing of which appears to be similar. In examining the body of a soldier, who had died of complicated disease, I found the head of the colon and the cœcum exceedingly distended with air, and of a bright red colour, as if highly inflamed, whilst the ascending colon was unusually contracted. The air collected under water amounted to 36 cubic inches, and consisted of 11 carbonic acid gas, or air absorbable by lime-water, and of 89, chiefly azote, judging from its extinguishing flame, and not being itself inflammable. I regret I had not the means of ascertaining if any traces of carburetted hydrogene were present.

The question, whether mucous membranes are capable of absorbing gases, I need not say is one of great importance in relation to the theory of respiration, and on that account

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* Ann. de Chim. et Phys. ii. 292.

deserving of particular attention. The theory which is now most generally adopted, is recommended by its simplicity, but is not well supported by the analogies and facts of physiology, which seem to favour the doctrine of the absorption of oxygene into the blood, and the evolution of carbonic acid; and *that,* perhaps, not in the air-cells of the lungs alone, but likewise along the whole tract of the primæ viæ, and over the whole of the external surface of the body.

Fort Pitt, Chatham, June 17, 1823.

XXX. *On Bitumen in Stones. By the Right Honourable* GEORGE KNOX, *F. R. S.*

Read June 12, 1823.

IN a Paper which I had the honour of presenting to the Royal Society on the 9th of May, 1822, I stated my intention of proceeding with an inquiry into the existence of bitumen in certain stones, not generally supposed to contain such a substance. The result of the investigation, I now beg leave to submit.

In the Paper referred to, I described the manner in which the pulverised stone was distilled, in order to obtain the volatile ingredients. I had at that time detected a bituminous substance in two varieties of Pitch-stone, the Newry, and the Meissen. I have since subjected to the same process the following minerals, viz., 1. Arran Pitch-stone; 2. Pearl-stone, from Tokay, in Hungary; 3. Pumice, from Iceland; 4. Amygdaloid, from Disco Island; 5. Basaltic, or Secondary Green-stone, from Newry; 6. Transition Green-stone, from Carlingford Mountain in the County of Louth; 7. Bole, from Disco Island; 8. Basalt, from the Giants' Causeway; 9. Basalt, from Disco Island; 10. Transition, by some considered Primitive Green-stone, from Clack Hill, near Castle Wellan, in the County of Down, Ireland; 11. Wacke, from Disco Island; 12. Iron Clay, from Disco Island; 13. Iron Clay, from Howth; 14. Hornblende, from Schneeburg, Upper Saxony; 15. Tourmaline, from Karorulik, in Greenland;

16. Augite, from Arendal, Norway; 17. Serpentine, from Zöplitz, Upper Saxony; 18. Clay Slate, from Bangor, North Wales; 19. White Felspar, from Killiney, near Dublin; 20. Flesh-red Felspar, from Aberdeen, Scotland; 21. Menilite, from Menil Montant, near Paris; 22. Adhesive Slate, from Menil Montant; 23. Mica Slate, from Freyburg, Saxony; 24. Mica, from the Ural Mountains, Siberia; 25. Obsidian, from the Lipari Islands; 26. Fetid Quartz, from Nantes in France; 27. Common Quartz; 28. Rock Crystal, from the Cape of Good Hope.

How far the investigation has been successful, or otherwise, the following details will determine.

1. *Arran Pitch-stone.*

This specimen was of a very dark oil-green colour, passing into raven black; the fracture conchoidal in all directions, with numerous dots of pearl-grey felspar.

It lost by ignition in a platina crucible 4,7059 per cent.; at a higher heat it fused, and lost on the whole 5 grains; when distilled in an iron retort 4,5 per cent. of water and bitumen came over: as nearly as I could estimate 2 per cent. was bitumen. It appeared to be similar to what I had obtained from the Newry Pitch-stone.

The retort contained a substance resembling pumice, but not sufficiently indurated. It broke into joints like those in basaltic columns, one end of each joint being convex and the other concave. The colour changed to milk-white.

2. *Pearl-stone.*

100 grains lost by ignition 3,25, and changed from ash-grey to reddish white, having probably gained oxygen.

When distilled, the product was 2,5 of water and floating bitumen. The substance remaining in the retort was like No. 1, a soft pumice, and broke in the same manner.

3. *Pumice.*

Fused without loss.

4. *Amygdaloid, from Disco Island.*

Lost by ignition 3,25.

The product of distillation was a bituminous water weighing 3,1 per cent.

5. *Basaltic Green-stone.*

This is the Green-stone which forms beds in granite near the Newry Pitch-stone, and which I mistook for basalt. The beds contain spheroidal concentric balls, as mentioned in my former Paper, and the specimen operated on was from one of those balls.

It lost by ignition 6,25 per cent. and produced by distillation, after the water had been expelled by a heat below redness, 1,75 of pure bitumen.

6. *Transition Green-stone, Carlingford.*

Lost by ignition 2 per cent. Distillation produced 1,5 per cent. chiefly bitumen.

7. *Bole.*

Lost by ignition 24,5 per cent. The colour changed from Isabella-yellow to tile-red.

A considerable quantity of bitumen was produced by distillation, but in consequence of an accident it could not be ascertained. It had a saline taste, reddened turmeric paper, and, when presented to the vapour of muriatic acid, dense fumes were formed.

8. *Basalt, from the Giants' Causeway.*

Lost by ignition 6,051 per cent.

Obtained by distillation 6 per cent. of bitumen and water. The retort contained a pumice, which broke, as in No. 1.

9. *Basalt, from Disco Island.*

By distillation, 2,312 per cent. of bitumen and water. The mass in the retort a pumice, which broke, as No. 1.

10. *Obsidian.*

By ignition lost 1,75 per cent. Colour changed from ash-grey to reddish-white. The original specimen, in the lump, raven black.

Distillation produced 0,2 per cent. of bituminous water, with indications of ammonia. The mass in the retort was a very vesicular, light, imperfectly vitrified substance, resembling the glassy pumice which adheres to, and is disseminated through, the Obsidian of Ascension Island. It broke as No. 1, and one piece, similar to the rest, had sublimed in the neck of the retort.

11. *Green-stone, from Clack Hill.*

By ignition lost 2 per cent.

Distillation produced 2 per cent. of bituminous water, chiefly bitumen; part of the bitumen so volatile as to be evaporated by the heat of the hand, through a thick glass, in a few seconds.

12. *Wacke, from Disco Island.*

This mineral, which is pure wacke, is found at Inmarsoall, on the south coast of Disco Island, midway between the east

and west boundary, imbedded in basaltic tuff, very near the shore. In the neighbourhood are beds of brown coal.

Lost by ignition 19,4 per cent.

Produce of distillation 11,42 bituminous water, 4 cubic inches of carbonic acid, and 8 of carburetted hydrogen.

The retort contained a velvet black powder, which being transferred to a covered platina crucible and exposed to a heat which melted cast iron, remained unchanged.

The carbone being burned off in an open vessel, there was a loss of 4 per cent. When the carbon was removed it melted into a slag.

I was induced by the above experiments, to try whether, by igniting this substance under charcoal to prevent the escape of the carbon, I might not make black chalk. The experiment was successful. The chalk was of a pretty good consistency and colour, and was effaced like graphite, both by bread and caoutchouc.

13. *Iron Clay, from Disco Island.*

By ignition it lost 21 per cent.

Distillation produced a bituminous water, weighing 18,25 per cent.

14. *Iron Clay, from Howth.*

By ignition lost 5 per cent.

Distillation produced 4 per cent of bitumen, with very little water. The mass in the retort was a pumice, but approaching to vitrification, and of a pale violet-blue colour: when exposed to a stronger heat in a platina crucible the colour changed to greenish-grey.

15. *Bole, from Disco Island.*

Distilled in a coated green glass retort, the result was nearly the same as No. 7, but it contained no ammonia.

Boiled a portion for a couple of hours in strong muriatic acid, filtered and evaporated. A large quantity of the mineral had been taken up by the acid, but no muriate of ammonia was formed.

16. *Hornblende, from Schneeburg, Upper Saxony.*

By ignition lost 3 per cent. and colour heightened.

Distillation produced 0,75 per cent. of bituminous water, but some was lost in the process.

17. *Tourmaline, from Karorulik, West Greenland.*

Distillation produced 0,7 per cent. of bituminous water. The mass in the retort was a pumice, but too much indurated. The stone was velvet-black, the powder ash-grey, and the pumice pearl-white.

18. *Augite, from Arendal.*

By ignition lost 0,5 per cent.

Distillation produced 0,35 of bituminous water, chiefly bitumen. The contents of the retort, a powder. The heat was not very great.

19. *Common Serpentine, from Zöplitz, Upper Saxony.*

By ignition lost 12,5 per cent.

Distillation produced 10,5 of bituminous water.

20. *Clay Slate, from Bangor.*

Lost by ignition, 3,25 per cent.

Distillation produced 3 per cent of bituminous water.

The contents of the retort were coherent, but not hard, and broke as No. 1.

21. *White Felspar, from Killiney.*

By ignition lost 0,40 per cent. colour unchanged.

Distillation in an iron retort produced 0,35 per cent. of bituminous water, which gave with the vapour of muriatic acid indications of ammonia.

22. *White Felspar, from Killiney.*

Distilled in a coated green-glass retort, same result, but no appearance of ammonia.

23. *Flesh-red Felspar, from Aberdeen.*

Distilled in a coated green-glass retort, a bituminous water produced, but by the melting of the retort a part was lost. It contained no ammonia. The contents of the retort were pale reddish-white, coherent, but friable.

24. *Menilite, from Menil Montant.*

Distillation produced 3,75 per cent. of bituminous water, with some ammonia. A friable substance remained in the retort, which on being exposed to a stronger heat in a platina crucible, lost 1,25 per cent.; probably carbon.

Ignited under charcoal it became black, but did not write.

25. *Adhesive Slate.*

This was the matrix of Menilite, from Menil Montant.

Distillation produced 18,5 per cent. of water and bitumen; the latter wine-yellow, and floating on the surface of the water.

The mass in the retort was coherent, but friable, and broke as No. 1.

26. *Mica Slate.*

Ignited; lost 2 per cent. and colour heightened.

Distilled; a bituminous water very volatile, but no trace of ammonia; part of the product lost.

Residuum in the retort a heavy pumice, which broke as No. 1.

27. *Mica, Silver-white.*

Distillation produced 1,33 per cent. of bituminous water, with indications of ammonia. The contents of the retort was slightly coherent.

28. *Fetid Quartz.*

By ignition lost 0,937 per cent. and its smell.

Distillation produced a bituminous water, smelling strongly of naphtha, but in which neither ammonia nor sulphuretted hydrogen could be traced.

29. *Pearl-white Common Quartz Fat Quartz.*

Ignition gave 1 per cent. loss.

By distillation 0,1 of a very fetid bituminous water.

Contents of the retort a powder unaltered in colour.

30. *Rock Crystal.*

By ignition lost nothing; the specimen was perfectly transparent and colourless, the powder snow-white.

31. *Adularia, a Crystal, Pearl-white.*

By ignition lost nothing.

32. *Pearl-blue Adularia, from Greenland.*

By ignition lost 0,4 per cent.

All these substances, with the exception of the Rock Crystal and Pearl-white Adularia, scintillated more or less strongly when projected on boiling nitre.

ADDITIONAL EXPERIMENTS.

I have again distilled felspar, and again obtained the volatile fluid.

33. *Carrara Marble.*

0,15 per cent. of water without smell, or alkaline mixture.

34. *Lucullite, from Galway.*

0,188 per cent.; an oily smell at first, but afterwards became ammoniacal. Litmus paper reddened by acetous acid became blue. Vegetable blue paper became green.

The contents of the retort white, except at the upper part, where there was a carbonaceous appearance; no effervescence, nor smell of sulphuretted hydrogen when put into dilute muriatic acid.

The distillations in iron retorts.

In a former distillation of Lucullite, in which the lime was not rendered quite caustic, muriatic acid in dissolving the residuum produced a smell of sulphuretted hydrogen.

⁂ I am obliged to Sir CHARLES GIESECKE, for the Greenland specimens; to Mr. GRIFFITH, for those from Arran, Carlingford, and Castle Wellan; and to Mr. MOORE, for the fetid quartz.

The stones were all ignited in a platina crucible.

OBSERVATIONS.

I do not mean to waste the time of the Royal Society, by applying the facts above stated to confirm, or invalidate, either of the great rival theories; still less to support any hypothesis of my own.

A most instructive lesson has been inculcated by the recent discoveries of Sir Humphry Davy, in his examination of the cavities of crystals. In that ingenious Paper, a fact, which had for many years been considered as evidence almost conclusive in favour of one system, has been converted into an argument nearly irresistible in support of the other. I shall confine myself, therefore, to calling the attention of the Society to some of the most obvious results and inferences.

It may be a question, whether the bitumen obtained by distillation, has actually existed in the stone. That it may have been somewhat altered, or contaminated in the process, is not improbable; but it is to be observed, that an inflammable oily substance, scarcely discernible from that which distils over, may be obtained in the common method of analysis; and likewise, that the bitumens obtained by distillation from such a variety of earthy substances, possess the same smell, colour, and volatility.

That the ammonia, however, which sometimes appears, is a *product*, and not an *educt*, I have myself little doubt; and I trust, it is rendered highly probable by the experiments on felspar and bole. I conceive that it arises from the decomposition of the bitumen, either by the iron of the retort, or the carbon of the stone, at a high temperature.

The manner in which pulverised stones, which are both

bituminous and vitrifiable, agglutinate, and form substances resembling *pumice,* is a subject which may throw some light upon the natural formation of that substance.

The conversion of *obsidian* into a species of *pumice,* and the proof that it contains bitumen, will probably be considered as supporting the mineralogical arrangement, which places that curious substance in connection with pitch-stone.

As it appears from the facts here detailed that bitumen, or a volatile inflammable oil, exists in considerable proportion, and in chemical union, with all the rocks of the Floetz Trap formation, is it a far-fetched inference to consider that formation as the chief source, whatever its own origin may have been, of the *ejected* volcanic products?

The appearance of an inflammable substance in the lower or elder rocks, such as mica, slate, &c. and, in particular, the exception in favour of colourless rock crystal and adularia, will probably obtain the attention of geologists.

It is observable also, that in the last named rocks, the quantity of the volatile and inflammable ingredients is less than in the upper or more recent formations, and that it seems also to be more firmly united.

If the scintillation of pulverised stones, when projected upon boiling nitre, be a test of their containing carbon, the experiments made with that object, demonstrate how much more generally that inflammable substance is distributed through the mineral kingdom, than was supposed; and may account for much of the loss which the best chemists and most experienced manipulators are often obliged to acknowledge in their analyses.

From these observations may it not be inferred, that no

analysis of a stone can be perfectly relied upon, unless the stone itself has been distilled, and the product of the distillation examined.*

To call the whole volatile matter which escapes when the stone has been ignited, *water*, is evidently a misnomer. How many stones must we not now expect to contain bitumen as well as water? From how many does not carbon escape in the shape of carbonic acid gas or carburetted hydrogen?

In conclusion, I hope I may be allowed, from what is above stated, to recommend a previous distillation in all analyses of stony substances, in order to obtain the liquid bitumen, and also the carbon which has escaped in the shape of gas: and that the residuum in the retort should be afterwards examined for the remaining carbon, either by burning it off, or in such other manner as may seem best to the operator.

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* In these distillations a matter often condensed in the retort, which was exceedingly volatile, and which was easily raised in vapour by the heat of the hand.

XXXI. *On certain changes which appear to have taken place in the positions of some of the principal fixed Stars. By* JOHN POND, *Astronomer Royal, F. R. S.*

Read June 19, 1823.

SINCE the date of my last communication on the subject of the deviation of the fixed stars from their computed or predicted places, I have been induced to examine such intermediate observations, as appeared likely to throw some light on this difficult subject.

The observations that best deserve attention since the time of BRADLEY, are the few which were made by the French astronomers, in their Trigonometrical Operations, about the year 1793, and those of Greenwich, Armagh, Westbury and Palermo, some years later, as published in the Philosophical Transactions for the year 1806. As the computations and tables relating to this investigation are subjoined, it will only be requisite, briefly, to state the result. It appears to me, that these observations greatly add to the probability that some variation, either continued or periodical, takes place in the sidereal system, which, producing but very small deviations in a finite portion of time, has hitherto escaped notice.

That in consequence of this, it becomes impossible, even if two perfectly exact observations of a star could be made at distant intervals, either by interpolation to assign its place for any intermediate period, or to predict its place for the future, contrary to the theory hitherto received. The nature

of this motion appears to be such, that the stars are now mostly found a considerable quantity to the southward of their computed or predicted places. With respect to the laws by which these motions are governed, the observations in question are not sufficiently exact to throw any light upon them.

Upon this very difficult point we must, I am inclined to think, rely chiefly, if not entirely, on the Greenwich observations; and as I have already fully discussed this question in my former Paper, I am unwilling, particularly at this advanced period of the season, to trespass any longer on the attention of the Royal Society.

TABLE I.

		Greenwich. 1800.	Armagh.	Palermo.	Westbury.	Westbury, 2 feet Circle.	Promiscuous Observations.	Mean of 4 Catalogues.
		° ′ ″	″	″	″	″	″	° ′ ″
1	Polaris							
2	β Ursæ Min.							
3	β Cephei							
4	α Ursæ Maj.							
5	α Cephei							
6	α Cassiop.							
7	γ Ursæ Maj.							
8	γ Draconis	38 28 54,0	54,0	54,0	54,0			38 28 54,0
9	η Ursæ Maj.							
10	α Persei							
11	Capella	44 13 21,5	21,5	21,0	18,7		18,0	44 13 20,1
12	α Cygni	45 25 41,4	39,5	38,7	37,2			45 25 39,2
13	α Lyræ	51 23 41,1*	37,3	37,7	36,2		35,0	51 23 36,5
14	Castor	57 41 15,0	* 9,5	13,5	14,2			57 41 14,2
15	Pollux	61 30 10,9	* 5,3	11,7	13,9			61 30 12,2
16	β Tauri	61 34 32,1	32,5	33,0	33,9			61 34 32,9
17	α Androm.	62 0 47,0	45,2	49,5	50,2			62 0 48,0
18	α Cor. Bor.	62 36 11,7	* 7,5	10,5	13,2			62 36 11,8
19	α Arietis	67 29 21,8	23,5	22,0	20,6			67 29 22,0
20	Arcturus	69 46 10,8	11,2	10,2	7,7			69 46 10,0
21	Aldebaran	73 54 20,0	18,5	16,5	15,7			73 54 17,7
22	β Leonis	74 18 37,9	34,2	33,8	32,7			74 18 34,7
23	α Herculis							
24	α Pegasi	75 52 0,5	1,3	1,4	58,2			75 52 0,2
25	γ Pegasi	75 55 39,8	37,7	*41,9	37,2			75 55 38,2
26	Regulus	77 3 38,8	* 32,2	36,5	34,2			77 3 36,5
27	α Ophiuchi	77 16 57,8	53,6	55,5	53,7			77 16 55,1
28	α Aquilæ	81 38 56,2	50,8	53,3	51,7			81 38 53,0
29	α Orionis	82 38 35,0	32,0	33,5	31,7			82 38 33,0
30	α Serpentis	82 56 5,4	* 0,0	4,8	2,4			82 56 4,2
31	Procyon	84 16 21,7	19,5	20,0	21,7	22,5		84 16 21,1
32	α Ceti	86 42 10,4	7,5	9,7	10,4			86 42 9,5
33	α Aquarii	91 17 4,6	4,8	4,6	4,8			91 17 4,7
34	α Hydræ	97 47 54,3	49,0	53,0	53,2			97 47 52,4
35	Rigel	98 26 34,2	33,2	34,9	36,7			98 26 34,7
36	Spica Virg.	100 6 42,7	39,0	42,5	43,2			100 6 41,9
37	2 α Capricor.	103 9 9,1	13,5	8,7	8,2			103 9 9,9
38	Sirius	106 27 2,3	5,3	4,7	2,2	3,0	1,5	106 27 3,1

* These are omitted in the calculations.

TABLE II.

		N.P.D. 1756. ° ′ ″ Co-Lat. 38 31 21,0	N.P.D. 1800. Greenwich.	Motion in 44 Years.	Motion in 22 Years.	Correction.	Ann. Var. 1811 × 22.	Predicted N.P.D. 1822.	Stars observed South of predicted Places.
		° ′ ″	° ′ ″	′ ″	′ ″	″	′ ″	° ′ ″	″
1	Polaris								
2	β Ursæ Min.	14 50 49,0							
3	β Cephei	20 30 22,9							
4	α Ursæ Maj.	26 56 16,7							
5	α Cephei	28 26 28,7							
6	α Cassiop.	34 48 17,4							
7	γ Ursæ Maj.	34 56 56,4							
8	γ Draconis	38 28 21,2	38 28 54,0	+ 0 32,8	+0 16,4	—1,5	+0 14,9	38 29 8,9	+ 0,9
9	η Ursæ Maj.	39 27 40,2							
10	α Persei	41 4 47,8							
11	Capella*	44 16 51,0	44 13 21,5	— 3 29,5	—1 44,7	—4,6	—1 40,1	44 11 41,4	0,0
12	α Cygni*	45 34 51,9	45 25 41,4	— 9 10,5	—4 35,2	+1,7	—4 36,9	45 21 4,5	+ 0,6
13	α Lyræ*	51 25 46,6	51 23 41,1	— 2 5,5	—1 2,8	+2,2	—1 5,0	51 22 36,0	— 1,8
14	Castor	57 36 10,2	57 41 15,0	+ 5 4,8	+2 32,4	+3,8	+2 36,2	57 43 51,2	+ 0,8
15	Pollux	61 24 27,9	61 30 10,9	+ 5 43,0	+2 51,5	+3,6	+2 55,1	61 33 6,0	+ 3,0
16	β Tauri	61 37 30,4	61 34 32,1	— 2 58,3	—1 29,1	—4,0	—1 25,1	61 33 7,0	+ 3,4
17	α Androm.	62 15 26,8	62 0 47,0	—14 39,8	—7 19,9	0,0	—7 19,9	61 53 27,1	+ 5,4
18	α Cor. Bor.	62 26 59,7	62 36 11,7	+ 9 12,0	+4 36,0	—2,1	+4 33,9	62 40 45,6	+ 2,6
19	α Arietis	67 42 12,4	67 29 21,8	—12 50,6	—6 25,3	—1,7	—6 23,6	67 22 58,2	+ 3,5
20	Arcturus	69 32 13,2	69 46 10,8	+13 57,6	+6 58,8	—1,5	+6 57,3	69 53 8,1	+ 2,1
21	Aldebaran	74 0 14,9	73 54 20,0	— 5 54,9	—2 57,4	—3,4	—2 54,0	73 51 26,0	— 0,5
22	β Leonis	74 3 55,1	74 18 37,9	+14 42,8	+7 21,4	+0,3	+7 21,7	74 25 59,6	— 1,4
23	α Herculis	75 18 45,6	75 22 7,1	+ 3 21,5	+1 40,7	—2,9	+1 37,8	75 23 44,9	+10,7
24	α Pegasi	76 6 10,2	75 52 0,5	—14 9,7	—7 4,8	+0,9	—7 5,7	75 44 54,8	+ 6,4
25	γ Pegasi	76 10 25,7	75 55 39,0	—14 45,9	—7 22,9	0,0	—7 22,9	75 49 16,9	+ 5,4
26	Regulus	76 51 4,7	77 3 38,8	+12 34,1	+6 17,0	+1,8	+6 18,8	77 9 57,6	+ 0,4
27	α Ophiuchi	77 14 35,4	77 16 57,8	+ 2 22,4	+1 11,2	—3,0	—1 8,2	77 18 6,0	+ 1,4
28	α Aquilæ	81 45 27,3	81 38 56,2	— 6 31,1	—3 15,5	+2,9	—3 18,4	81 35 37,8	+ 0,9
29	α Orionis	82 39 41,8	82 38 35,0	— 1 6,8	—0 33,4	—3,4	—0 30,0	82 38 5,0	+ 0,7
30	α Serpentis	82 47 24,2	82 56 5,4	+ 8 40,8	+4 20,4	—2,5	—4 17,9	83 0 23,3	+ 1,7
31	Procyon	84 10 19,8	84 16 21,7	+ 6 11,9	+3 5,9	+3,1	+3 9,0	84 19 30,7	+ 3,9
32	α Ceti	86 52 57,7	86 42 10,4	—10 47,3	—5 23,6	—2,2	—5 21,4	86 36 49,0	+ 2,4
33	α Aquarii	91 29 41,4	91 17 4,6	—12 36,8	—6 18,4	+1,7	—6 20,1	91 10 44,5	+ 4,1
34	α Hydræ	97 36 50,2	97 47 54,3	+11 4,1	+5 32,0	+2,0	—5 34,0	97 53 28,3	+ 1,0
35	Rigel	98 30 10,9	98 26 34,2	— 3 36,7	—1 48,3	—2,9	—1 45,4	98 24 48,8	+ 4,4
36	Spica Virg.	99 52 48,3	100 6 42,7	+13 54,4	+6 57,2	—1,1	+6 56,1	100 13 38,8	+ 3,0
37	2 α Capricor.	103 16 53,0	103 9 9,1	— 7 43,9	—3 51,9	+3,1	—3 55,0	103 5 14,1	+ 3,3
38	Sirius	106 23 56,7	106 27 2,3	+ 3 5,6	+1 32,8	+2,7	+1 35,5	106 28 37,8	+ 6,5

*** Dr. Maskelyne considered the determination of these three stars as erroneous, and assigned corrections amounting to two or three seconds—vide Greenwich Observations.

TABLE III.

		N. P. D. 1756. ° ′ ″ Co-Lat. 38 31,21,0	Westbury, N. P. D. 1800.	Motion in 44 Years.	Motion in 22 Years.	Correction.	Ann. Var. 1811 × 22.	Predicted N. P. D. 1822.	Stars observed South of predicted Places.
		° ′ ″	° ′ ″	″ ′	′ ″	″	′ ″	° ′ ″	″
1	Polaris								
2	β Ursæ Min.	14 50 49,0	15 1 36,8	+10 47,8	+ 5 23,9	+0,2	+5 24,1	15 7 0,9	— 0,1
3	β Cephei	20 30 22,9							
4	α Ursæ Maj.	26 56 16,7							
5	α Cephei	28 26 28,7							
6	α Cassiopeiæ	34 48 17,4							
7	γ Ursæ Maj.	34 56 56,4							
8	γ Draconis	38 28 21,2	38 28 53,6	+ 0 32,4	+ 0 16,2	—1,5	+0 14,7	38 29 8,3	+ 1,5
9	η Ursæ Maj.	44 16 51,0							
10	α Persei	45 34 51,9							
11	Capella	44 16 51,0	44 13 19,2	— 3 31,8	— 1 45,9	—4,6	—1 41,3	44 11 37,9	+ 3,5
12	α Cygni	45 34 51,9	45 25 37,8	— 9 14,1	— 4 37,0	+1,7	—4 38,7	45 20 59,1	+ 6,0
13	α Lyræ	51 25 46,6	51 23 36,7	— 2 9,9	— 1 5,0	+2,2	—1 7,2	51 22 29,5	+ 4,7
14	Castor	57 36 10,2	57 41 14,2	+ 5 4,0	+ 2 32,0	+3,8	+2 35,8	57 43 50,0	+ 2,0
15	Pollux	61 24 27,9	61 30 13,5	+ 5 45,6	+ 2 52,8	+3,6	+2 56,4	61 33 9,9	— 0,9
16	β Tauri	61 37 30,4	61 34 34,0	— 2 56,4	— 1 28,2	+4,0	—1 24,2	61 33 9,8	+ 0,6
17	α Androm.	62 15 26,8	62 0 49,3	—14 37,5	— 7 18,7	0,0	—7 18,7	61 53 30,6	+ 1,9
18	α Cor. Bor.	62 26 59,7	62 36 12,1	+ 9 12,4	+ 4 36,2	—2,1	+4 34,1	62 40 46,2	+ 2,0
19	α Arietis	67 42 12,4	67 29 21,8	—12 50,6	— 6 25,3	—1,7	—6 23,6	67 22 58,2	+ 3,5
20	Arcturus	69 32 13,2	69 46 9,4	+13 56,2	+ 6 58,1	—1,5	+6 56,6	69 53 6,0	+ 4,2
21	Aldebaran	74 0 14,9	73 54 17,1	— 5 57,8	— 2 58,9	—3,4	—2 55,5	73 51 21,6	+ 3,9
22	β Leonis	74 3 55,1	74 18 33,8	+14 38,7	+ 7 19,3	+0,3	+7 19,6	74 25 53,4	+ 4,8
23	α Herculis	75 18 45,6							
24	α Pegasi	76 6 10,2	75 51 59,9	—14 10,3	— 7 5,1	+0,9	—7 6,0	75 44 53,9	+ 7,3
25	γ Pegasi	76 10 25,7	75 55 38,7	—14 47,0	— 7 23,5	0,0	—7 23,5	75 48 15,2	+ 7,1
26	Regulus	76 51 4,7	*77 3 35,0	+12 30,3	+ 6 15,1	+1,8	+6 16,9	77 9 51,9	+ 6,1
27	α Ophiuchi	77 14 35,4	77 16 54,4	+ 2 19,0	+ 1 9,5	—3,0	+1 6,5	77 18 0,9	+ 6,5
28	α Aquilæ	81 45 27,3	81 38 52,8	— 6 34,5	— 3 17,2	+2,9	—3 20,1	81 35 32,7	+ 6,0
29	α Orionis	82 39 41,8	82 38 32,8	— 1 9,0	— 0 34,5	—3,4	—0 31,1	82 38 1,7	+ 4,0
30	α Serpentis	82 47 24,2	82 56 3,5	+ 8 39,3	+ 4 19,6	—2,5	+4 17,1	83 0 20,6	+ 4,4
31	Procyon	84 10 9,8	84 16 22,3	+ 6 12,5	+ 3 6,2	+3,1	+3 9,3	84 19 31,6	+ 3,0
32	α Ceti	86 52 57,7	86 42 10,1	—10 47,6	— 5 23,8	—2,2	—5 21,6	86 36 48,5	+ 2,9
33	α Aquarii	91 29 41,4	91 17 5,0	—12 36,4	— 6 18,2	+1,7	—6 19,9	91 10 45,1	+ 3,5
34	α Hydræ	97 36 50,2	97 47 54,5	+11 4,3	+ 5 32,1	+2,0	+5 34,1	97 53 28,6	+ 0,7
35	α Rigel	98 30 10,9	98 26 37,6	— 3 33,3	— 1 46,6	—2,9	—1 43,7	98 24 53,9	— 0,7
36	Spica Virg.	99 52 48,3	100 6 44,2	+13 55,9	+ 6 57,9	—1,1	+6 56,8	100 13 41,0	+ 0,8
37	2 α Capricor.	103 16 53,0	103 9 10,3	— 7 42,7	— 3 51,3	+3,1	—3 54,4	103 5 15,9	+ 1,5
38	Sirius	106 23 56,7	106 27 4,3	+ 3 7,6	+ 1 33,8	+2,7	+1 36,5	106 28 40,8	+ 3,5

* This determination of Regulus was from two imperfect observations only, and is therefore probably erroneous.

TABLE IV.

		N. P. D. 1756. ° ′ ″ Co. Lat. 38 31 21,0	N. P. D. 1800. Mean of 4 Catalogues.	Motion in 44 years.	Motion in 22 years.	Corrections.	Ann. Var 1811 × 22.	Predicted N. P. D. 1822.	Stars observed South of predicted Places.
		° ′ ″	° ′ ″	′ ″	′ ″	″	′ ″	° ′ ″	
1	Polaris.								
2	β Ursæ Min.	14 50 49,0							
3	β Cephei	20 30 22,9							
4	α Ursæ Maj.	26 56 16,7							
5	α Cephei	28 26 28,7							
6	α Cassiopeiæ	34 48 17,4							
7	γ Ursæ Maj.	34 56 56,4							
1	γ Draconis	38 28 21,2	38 28 54,0	+ 0 32,8	+ 0 16,4	−1,5	+0 14,9	38 29 8,9	+ 0,9
9	η Ursæ Maj.	39 27 40,2							
10	α Persei	41 4 47,8							
11	Capella	44 16 51,0	44 13 20,1	− 3 30,9	− 1 45,4	−4,6	−1 40,8	44 11 39,3	+ 2,1
12	α Cygni	45 34 51,9	45 25 39,2	− 9 12,7	− 4 36,3	+1,7	−4 38,0	45 21 1,2	+ 3,9
13	α Lyræ	51 25 46,6	51 23 36,5	− 2 10,1	− 1 5,0	+2,2	−1 7,2	51 22 29,3	+ 4,9
14	Castor	57 36 10,2	57 41 14,2	+ 5 4,0	+ 2 32,0	+3,8	+2 35,8	57 43 50,0	+ 2,0
15	Pollux	61 24 27,9	61 30 12,2	+ 5 44,3	+ 2 52,1	+3,6	+2 55,7	61 33 7,9	+ 1,1
16	β Tauri	61 37 30,4	61 34 32,9	− 2 57,5	− 1 28,7	−4,0	−1 24,7	61 33 8,2	+ 2,2
17	α Androm.	62 15 26,8	62 0 48,0	−14 38,8	− 7 19,4	0,0	−7 19,4	61 53 28,6	+ 3,9
18	α Cor. Bor.	62 26 59,7	62 36 11,8	+ 9 12,1	+ 4 36,0	−2,1	+4 33,9	62 40 45,7	+ 2,5
19	α Arietis	67 42 12,4	67 29 22,0	−12 50,4	− 6 25,2	−1,7	−6 23,5	67 22 58,5	+ 3,2
20	Arcturus	69 32 13,2	69 46 10,0	+13 56,8	+ 6 58,4	−1,5	+6 56,9	69 53 6,9	+ 3,3
21	Aldebaran	74 0 14,9	73 54 17,7	− 5 57,2	− 2 58,6	−3,4	−2 55,2	73 51 22,5	+ 3,0
22	β Leonis	74 3 55,1	74 18 34,7	+14 39,6	+ 7 19,8	+0,3	−7 20,1	74 25 54,8	+ 3,4
23	α Herculis	75 18 45,6							
24	α Pegasi	76 6 10,2	75 52 0,2	−14 10,0	− 7 5,0	+0,9	−7 5,9	75 44 54,3	+ 6,9
25	γ Pegasi	76 10 25,7	75 55 38,7	−14 47,5	− 7 23,7	0,0	−7 23,7	75 48 14,5	+ 7,8
26	Regulus	76 51 4,7	77 3 36,5	+12 31,8	+ 6 15,9	+1,8	+6 17,7	77 9 54,2	+ 3,8
27	α Ophiuchi	77 14 35,4	77 16 55,1	+ 2 19,7	+ 1 9,8	−3,0	+1 6,8	77 18 1,9	+ 5,5
28	α Aquilæ	81 45 27,3	81 38 53,0	− 6 34,3	− 3 17,1	+2,9	−3 20,0	81 35 33,0	+ 5,7
29	α Orionis	82 39 41,8	82 38 33,0	− 1 8,8	− 0 34,4	−3,4	−0 31,0	82 38 2,0	+ 3,7
30	α Serpentis	82 47 24,2	82 56 4,2	+ 8 40,0	+ 4 20,0	−2,5	+4 17,5	83 0 21,7	+ 3,3
31	Procyon	84 10 9,8	84 16 21,1	+ 6 11,3	+ 3 5,6	+3,1	+3 8,7	84 19 29,8	+ 4,8
32	α Ceti	86 52 57,7	86 42 9,5	−10 48,2	− 5 24,1	−2,2	−5 21,9	86 36 47,6	+ 3,8
33	α Aquarii	91 29 41,4	91 17 4,7	−12 36,7	− 6 18,3	+1,7	−6 20,0	91 10 44,7	+ 3,9
34	α Hydræ	97 36 50,2	97 47 52,4	+11 2,2	+ 5 31,1	+2,0	+5 33,1	97 53 25,5	+ 3.8
35	Rigel	98 30 10,9	98 26 34,7	− 3 36,2	− 1 48,1	−2,9	−1 45,2	98 24 49,5	+ 3.7
36	Spica Virg.	99 52 48,3	100 6 41,9	+13 53,6	+ 6 56,8	−1,1	+6 55,7	100 13 37,6	+ 4,3
37	2 α Capricor.	103 16 53,0	103 9 9,9	− 7 43,1	− 3 51,6	+3,1	−3 54,7	103 5 15,2	+ 2,2
38	Sirius	106 23 56,7	106 27 3,1	+ 3 6,4	+ 1 33,2	+2,7	+1 35,9	106 28 39,0	+ 5,3

TABLE V.

		N. P. D. 1800. Interpolated.	N. P. D. 1813. Interpolated.	Ann. Var. 1795.	N. P. D. 1790.	Ann. Var. 1785.	N. P. D. 1780.
		° ′ ″	° ′ ″	″	° ′ ″	″	° ′ ″
1	α Cassiopeiæ	34 33 42,4	34 29 24,2	—19,87	34 37 1,1	—19,87	34 40 19,8
2	Polaris		1 41 21,9				
3	α Arietis	67 29 24,3	67 25 38,1	—17,42	67 32 18,5	—17,44	67 35 12,9
4	α Ceti	86 42 11,8	86 39 2,3	—14,62	86 44 38,0	—14,65	86 47 4,5
5	α Persei	40 51 45,7	40 48 53,5	—13,54	40 54 1,1	—13,58	40 56 16,9
6	Aldebaran	73 54 19,7	73 52 36,6	— 7,99	73 55 39,6	— 8,04	73 57 0,0
7	Capella	44 13 21,1	44 12 21,9	— 4,66	44 14 7,7	— 4,72	44 14 54,9
8	Rigel	98 26 37,2	98 25 35,4	— 4,79	98 27 25,1	— 4,83	98 28 13,4
9	β Tauri	61 34 32,9	61 33 44,3	— 3,90	61 35 11,9	— 3,96	61 35 51,5
10	α Orionis	82 38 35,5	82 38 17,4	— 1,43	82 38 49,8	— 1,48	82 39 4,6
11	Sirius	106 27 6,8	106 28 4,1	+ 4,38	106 26 23,0	+ 4,34	106 25 39,6
12	Castor	57 41 15,8	57 42 47,7	+ 7,01	57 40 5,7	+ 6,96	57 38 56,1
13	Procyon	84 16 24,8	84 18 16,7	+ 8,57	84 14 59,1	+ 8,53	84 13 33,8
14	Pollux	61 30 13,3	61 31 56,8	+ 7,90	61 28 54,3	+ 7,85	61 27 35,8
15	α Hydræ	97 47 55,0	97 51 12,3	+15,16	97 45 23,4	+15,13	97 42 52,1
16	Regulus	77 3 39,4	77 7 23,2	+17,19	77 0 47,5	+17,17	76 57 55,8
17	α Ursæ Maj.	27 10 22,7	27 14 31,1	+19,21	27 7 10,6	+19,20	27 3 58,6
18	β Leonis	74 18 37,0	74 22 57,6	+20,04	74 15 16,6	+20,04	74 11 56,2
19	γ Ursæ Maj.	35 11 35,3	35 15 55,0	+19,97	35 8 15,6	+19,97	35 4 55,9
20	Spica Virg.	100 6 44,6	100 10 51,2	+18,98	100 3 34,8	+19,00	100 0 24,8
21	η Ursæ Maj.	39 41 1,9	39 44 58,0	+18,17	39 38 0,2	+18,19	39 34 58,3
22	Arcturus	69 46 12,3	69 50 19,3	+19,04	69 43 1,9	+19,06	69 39 51,3
23	β Ursæ Min.	14 59 29,3	15 4 48,4	+14,72	14 57 2,1	+14,72	14 54 34,9
24	α Cor. Bor.	62 36 13,6	62 38 56,0	+12,53	62 34 8,3	+12,56	62 32 2,7
25	α Serpentis	82 56 6,5	82 58 39,4	+11,80	82 54 8,5	+11,84	82 52 10,1
26	Antares	115 58 25,1	116 0 10,9	+ 8,73	115 56 57,8	+ 8,78	115 55 30,0
27	α Herculis	75 22 14,1	75 23 14,4	+ 4,68	75 21 27,3	+ 4,72	75 20 40,1
28	α Ophiuchi	77 16 58,9	77 17 40,0	+ 3,19	77 16 27,0	+ 3,23	77 15 54,7
29	γ Draconis	38 28 54,6	38 29 3,7	+ 0,72	38 28 47,4	+ 0,74	38 28 40,0
30	α Lyræ	51 23 38,9	51 23 1,1	— 2,93	51 24 8,2	— 2,90	51 24 37,2
31	α Aquilæ	81 38 56,8	81 36 59,9	— 8,12	81 40 18,0	— 8,09	81 41 38,9
32	α Cygni	45 25 41,8	45 22 58,4	—12,54	45 27 47,2	—12,52	45 29 52,4
33	α Cephei	28 15 28,7	28 12 13,3	—15,02	28 17 58,9	—15,00	28 20 28,9
34	β Cephei	20 6 22,2	20 15 30,8	—15,66	20 8 58,8	—15,65	20 11 35,2
35	α Aquarii	91 17 7,4	91 13 23,7	—17,17	91 19 59,1	—17,15	91 22 50,6
36	α Pegasi	75 52 4,8	75 47 54,3	—19,23	75 55 17,1	—19,22	75 58 29,3
37	α Andromed.	62 0 50,8	61 56 31,7	—19,92	62 4 10,0	—19,92	62 7 29,2

TABLE VI.

Names of Stars.	Bradley, 1756.	Mayer. 1756.	Mayer corrected.	Difference between Bradley and Mayer.
	° ′ ″	° ′ ″	″	″
Capella	44 16 51,0	44 16 49,3	52,8	—1,8
α Cygni	45 34 51,9	45 34 48,4	51,9	0,0
α Lyræ	51 25 46,6	43,6	47,1	—0,5
Castor	57 36 10,2	6,9	10,4	—0,2
Pollux	61 24 27,9	23,4	26,9	+1,0
β Tauri	61 37 30,4	27,7	31,2	—0,8
α Androm.	62 15 26,8	22,4	25,9	+0,9
α Arietis	67 42 12,4	7,1	12,6	—0,2
Arcturus	69 32 13,2	12,8	16,3	—3,1
Aldebaran	74 0 14,9	11,9	15,4	—0,5
β Leonis	74 3 55,1	53,9	57,4	—2,3
α Pegasi	76 6 10,2	5,2	8,7	+1,5
Regulus	76 51 4,7	0,0	3,5	+1,2
α Ophiuchi	77 14 35,4	32,6	36,1	—0,7
α Aquilæ	81 45 27,3	23,4	26,9	+0,4
α Orionis	82 39 41,8	41,1	44,6	—2,8
α Serpentis	82 47 24,2	18,3	21,8	+2,4
Procyon	84 10 9,8	8,7	12,2	—2,4
α Ceti	86 52 57,7	50,6	54,1	+3,6
α Aquarii	91 29 41,4	36,3	39,8	+1,6
α Hydræ	97 36 50,2	46,1	49,6	+0,6
Rigel	98 30 10,9	9,5	13,0	—2,1
Spica Virg.	99 52 48,3	45,2	48,7	—0,4
Sirius	106 23 56,7	54,2	57,7	—1,0
Antares	115 51 57,3	54,8	58,3	—1,0
γ Pegasi	76 10 25,7	22,1	25,6	+0,1
2 α Capricor.	103 16 53,0	49,0	52,5	+0,5

The French astronomers, in their Trigonometrical Operations, employed six stars, whose declinations for the year 1793, they determined with a singular degree of precision with their repeating circle, viz.

N. P. D.

		°	′	″
1793	α Draconis	25	37	52,05
	ζ Urs. Maj.	33	59	21,30
	Capella	44	13	50,40
	Pollux	61	29	15,20
	β Tauri	61	35	0,50
1796	β Urs. Min.	15	0	40,37[*]

By combining these with Bradley's observations in 1753, I compute their predicted places for 1823, and find them by observation as follows:

	″
β Urs. Min.	3,5 North of its predicted place.
α Draconis	3,5 North.
ζ Urs. Maj.	1,3 South.
Capella	6,0 South.
Pollux	4,0 South.
β Tauri	3,0 South.
Sirius	7,0 South, from a determination of Mechain with the repeating circle in 1800.

The above are the best authorities that can be found from the time of Bradley to the year 1813.

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[*] The Westbury determination of β Ursæ Minoris differs 2″ from this; and as the observations were made on the star both above and below the Pole, it merits some confidence. The mean of the French and Westbury Observations give a northern motion equal to 1″.7.

Computation of the Southern Motion of CAPELLA.

	° ′ ″	
1753. N. P. D. - - -	44 17 5,20	Extremely exact; from a computation by Dr. MASKELYNE, in his own hand-writing.
1793. N. P. D. - - -	44 13 50,40	Arc du Meridien, page 653. BRADLEY's refraction.
	3 14, 8	Motion in 40 years.

$\frac{194'',8}{40} =$ 4″,870 An. Var. in 1773.

Precession $\left\{ \begin{array}{l} 1773 = 5,193 \\ 1808 = 4,974 \end{array} \right\}$ diff. = 0,217 change of precession in 35 years.

4,653 An. Var. for 1808.

30

1395, 9 = 2′ 19″,6 predicted motion in 30 years.

	° ′ ″	
1793, N. P. D. - - -	44 13 50,4	
	2 19,6	
	44 11 30,8	predicted N. P. D. 1823.
	44 11 36,8	observed N. P. D. 1823.
	6,0	Star south of predicted place in 30 years.

Explanation of the preceding Tables.

Table I. is nearly the same as published in the Philosophical Transactions for 1806. The Greenwich Catalogue is corrected for flexure, and the other catalogues corrected each by a common quantity, so as to make the polar distance of γ Draconis 38° 28′ 54″, the same as in the Greenwich Catalogue.

It may be doubted whether the Palermo Catalogue can, with any propriety, be introduced in this investigation, considering that the observations were made in a different latitude, and computed by a different table of refraction. The discordances in these catalogues are very considerable, and show that very little reliance can be placed even on the most probable mean of them all.

Table II. shows the southern motion, as deduced from the Greenwich Catalogue of 1800, corrected for the flexure of the mural quadrant.

Table III. in the same manner, shows the southern motion deduced from the Westbury Catalogue.

Table IV. shows the southern motion deduced from the mean of all the Catalogues. From this Table it appears, that however doubtful the determination may be, as deduced from any particular star, yet the general tendency of motion to the southward is so obvious, as to leave but little room to doubt of its reality.

Table V. contains interpolated places for the years 1780, 1790, 1800, 1813. These Tables are formed upon the supposition that no southern motion exists, but the proper motions

of all the stars are uniform: it moreover supposes the present Catalogue for 1823 exact. It has been seen, that it is quite impossible to reconcile the very best catalogues to such a supposition; as has been particularly exemplified in the observations of Greenwich and Dublin for the year 1813. The Greenwich Observations for that year will be found very erroneous, and those of Dublin still more so. Indeed it appears to me that the Dublin observations cannot be placed in a more unfavourable point of view, than by supposing the southern motion in question not to exist.

TABLE VI. contains the Catalogues of MAYER and BRADLEY; the former is corrected by a common quantity $3'',5$, which I find necessary to equalize the positive and negative differences.

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